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About this issue . . .

The debate goes on. Matthew Landau exposes one of the errors in Michael Denton's critique of evolution, while Stephen J. Godfrey continues *Creation/Evolution*'s challenge to Robert V. Gentry's creationist conclusions from polonium halo research. The bulk of this issue, however, continues the spirited exchange between Jim Lippard and Walter T. Brown, Jr. We have received many letters on this but, due to space limitations, could publish only one in this issue's "Letters to the Editor" section. Additional letters will appear next issue.

In regard to this dialogue, we apologize for an omission in "Brown Responds to Lippard" on page forty-one of *Creation/Evolution* XXV. Two lines were inadvertently dropped from Brown's discussion of two- to twenty-celled life forms. The sentence, in its entirety, should have read:

> Libbie Henrietta Hyman has pointed out the many differences between colonial forms and multicellular forms of life—of *all* kingdoms. Multicellular forms have a much higher degree of cellular differentiation than colonial forms (Hyman, 1940, pp. 248–255).

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Protein Sequences and Denton's Error

Matthew Landau

One of the arguments I've heard from creationists during debates and presentations over the past few years is that analyses of protein sequences do not support the scientific view that one-celled organisms, invertebrates, fish, and tetrapods slowly evolved from each other *in this sequence*—the less complex forms increasing in complexity slowly over the eons. The source of this stunning pronouncement (at least, I was stunned when I first heard it) is Michael Denton's book *Evolution: A Theory in Crisis* (1985). When Denton's protein "data" are presented by a skilled debater, this argument sounds very plausible; in fact, it's not very good science and provides yet another example of the convoluted creationist reasoning we have become all too familiar with. Denton's book is rife with twisted data and half-truths, much of it old hat. Since a great deal of Denton's material has already been addressed in *Creation/Evolution* and elsewhere, I will restrict my comments to Denton's notions about protein sequences.

The technique of using protein sequences for re-creating evolutionary relationships is now over twenty years old. In principle, it is assumed that organisms with proteins of similar structure are more closely related than organisms with proteins that are very different. This is best explained by a simple example in which I have substituted small peptides (compounds containing two or more amino acids linked by carboxyl groups) for the larger proteins. Suppose you have sequenced a particular peptide in an organism designated as *M*. (By *sequenced*, biologists mean that they have determined the order of amino acids comprised by that peptide.) Then you sequence the same peptide in three other organisms: *N*, *O*, and *P* (see TABLE 1). We see that *N* has the same amino acids as peptide *M* at 90 percent of the sites. In addition, we see that *O* and *P* are 80 percent and 70 percent similar to *M*, respectively. Also note that the new amino acid that is seen in *N* is also seen in *O* and that both of the new amino acids seen in *O* are also seen in *P*. Based upon these results, one could reconstruct two possible histories for the four species, as seen in FIGURE 1. Which of these two histories is the correct

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	Percent Similarity to *M*
PEPTIDE *M* from a Bacterium:	
Ala—His—Gly—Tyr—Pro—Arg—Lue—Ala—Glu—His	—
PEPTIDE *N* from a Green Alga:	
Ala—His—*Asp*—Tyr—Pro—Arg—Lue—Ala—Glu—His	90
PEPTIDE *O* from a Fern:	
Ala—His—*Asp*—Tyr—Pro—*Val*—Lue—Ala—Glu—His	80
PEPTIDE *P* from a Tree:	
Ala—*Ala*—*Asp*—Tyr—Pro—*Val*—Lue—Ala—Glu—His	70

Table 1
The sequences of four similar peptides
(italics show where the sequences differ from peptide *M*)

one? Based only upon the amino acid data, it is impossible to say; however, normally other data (paleontological, morphological, developmental, and biochemical) are available. In my example, I have assigned peptide *M* to a bacterium, peptide *N* to a green alga, peptide *O* to a fern, and peptide *P* to a tree (see Table 1). Based upon a good deal of published data, we will assume that the history depicted in Figure 1(a) is more likely to be correct than the history depicted in Figure 1(b). Thus, Figure 1(a) indicates that, at some time in the past, species *M* and *N* shared a single common ancestor species but a speciation event resulted in two new species, which evolved slowly over time into the modern *M* and *N* species. Since the peptides *O* and *P* are more dissimilar from peptide *M* than peptide *N* is, we can assume that the ancestors of *O* and *P* branched away earlier from the line leading to *M*. It is very important to understand that, although *P* separated from the *O-N-M* group in the more distant past, it has continued to evolve since that split.

Denton generated a matrix of protein sequences from Dayhoff's *Atlas of Protein Sequence and Structure* (1972). Again, for the sake of simplicity, I have shortened the matrix a bit (see Figure 2, p. 4). The numbers in this matrix are the percentage of sequence *differences*; that is, a horse is zero percent different in cytochrome *C* sequence from itself but about 18 percent different in sequence from a tunafish. Denton calls the reader's attention to the last column, which shows the differences

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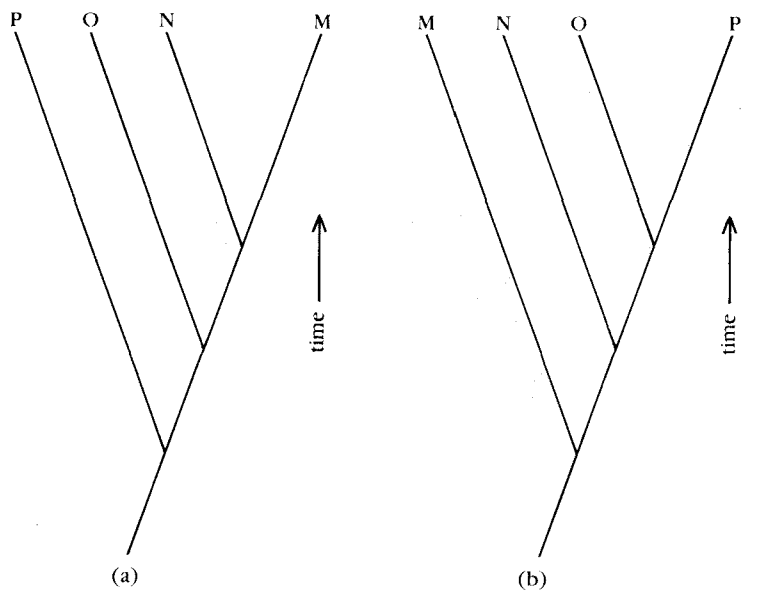

Figure 1
Two possible histories of the *M-N-O-P* group based upon peptide sequences

between a bacterium (a prokaryote) and other forms of life (all eukaryotes). The fact that the eukaryotes may vary greatly amongst themselves (for example, yeast and a silkmoth are 44 percent different) but are *all* 64 to 69 percent different from the prokaryote is very strange, according to Denton. He tells us that "no eukaryotic cytochrome is intermediate between the bacterial cytochrome and the other eukaryotic cytochromes." Denton has clearly impressed himself with this discovery, calling it "one of the most astonishing findings of modern science." He then generates a diagram (Figure 3, p. 5) to show us that all of the listed eukaryotes are approximately equidistant, in terms of cytochrome *C* structure, from the bacterium. Amazing—yes?

No! Not from the standpoint of evolutionary science. In fact, I would be very surprised to see anything else. Let's draw out a cladogram (sort of a family tree) of the organisms in the matrix based upon conventional evolutionary theory (see Figure 4, p. 6). Notice that at point *a* on the time line there was a speciation event separating the bacterium from the eukaryotes. Since that point in time, the bacterium has continued to evolve—and so have all the other organisms. If all the eukaryotes have evolved for the same amount of time (from point *a* to the present), it is not

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surprising that they are all about equidistant from the bacterium in terms of their cytochrome *C* sequences. Furthermore, note that the rest of the data in the matrix now make sense. The horse-pigeon difference is small (11 percent) because they split relatively recently (at point *f* on the time line), while the tunafish differs from the horse by 18 percent because it split from the horse-pigeon ancestor in the more distant past (at point *e*). In other words, *just what modern science would expect.*

Denton's book seems to suggest that even evolutionary scientists must hold humans as the ultimate "goal" of natural selection (in fact, evolutionary theory does *not* embrace the notion of a predetermined goal or direction). So, perhaps Denton's straw-man evolutionist also thinks that after the bacterium split with the yeast it ceased evolving because its job was done; that the evolution of the yeast halted after the yeast split with the wheat; and so on. If this were the case, the

	horse	pigeon	tuna	silkmoth	wheat	yeast	bacterium
horse	0	11	18	27	41	42	64
pigeon	11	0	17	25	41	41	64
tuna	18	17	0	30	44	43	65
silkmoth	27	25	30	0	40	44	65
wheat	41	41	44	40	0	42	66
yeast	42	41	43	44	42	0	69
bacterium	64	64	65	65	66	69	0

Figure 2
Matrix of cytochrome *C* similarities
(data from Dayhoff, 1972)

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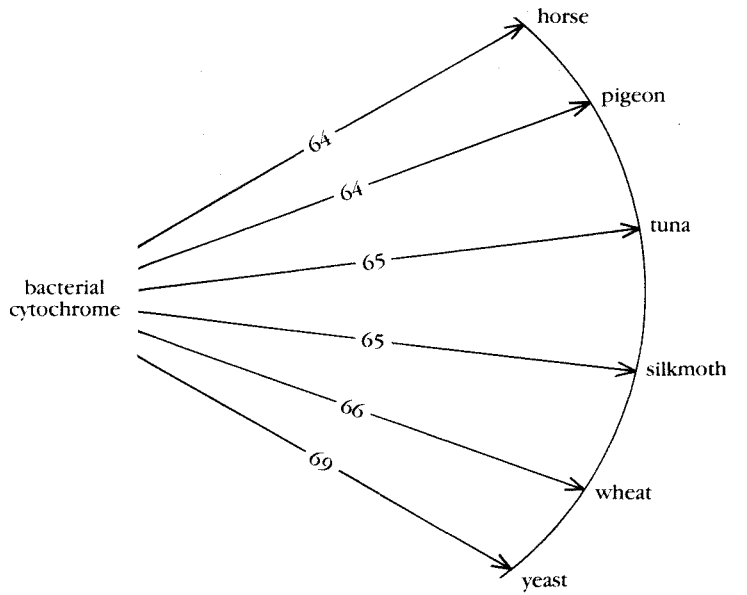

Figure 3

A comparison of bacterial-eukaryote cytochrome similarities.
(Redrawn from M. Denton, *Evolution: A Theory in Crisis*, 1985. *Evolution* is distributed by Woodbine House, 10400 Connecticut Avenue, Kensington, MD 20895. (800) 843-7323.)

wheat at point *c* on the time line would be exactly like the wheat of today. I suspect that Denton's evolutionist also believes that evolution is a *linear* rather than a *branched* sequence of events (compare Figure 5 with Figure 4). This would explain why Denton thought he had shocked the scientific community when he pointed out that the difference in the cytochrome *C* sequence between a bacterium and a wheat is 66 percent and the difference between that bacterium and a tunafish is almost the same. If things had evolved in a linear manner, with bacteria unchanging since point *a* on the time line, then certainly that bacterium should have a cytochrome *C* sequence somewhat similar to that of the wheat and much more dissimilar to that of the tunafish.

Denton's confusion in this respect is certainly shared by many other people. Perhaps some of this confusion is the fault of the textbooks that are presently used in our schools (many with little or no discussion of evolution). Perhaps some of it is attributable to our sloppy speech habits: we often hear yeast or bacteria referred to as "primitive" when what we really mean is that "modern" yeast and

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bacteria have evolved for millions of years to adapt to a changing world but have still retained some degree of their ancestor's simplicity. Whatever the reason, it seems obvious to me as a teacher that a large portion of the general public has come to accept the notion of linear evolution (for example, that humans evolved from the modern gorilla). We must not let books like Denton's reinforce this notion; we must not let creationists define evolution for the evolutionary scientist.

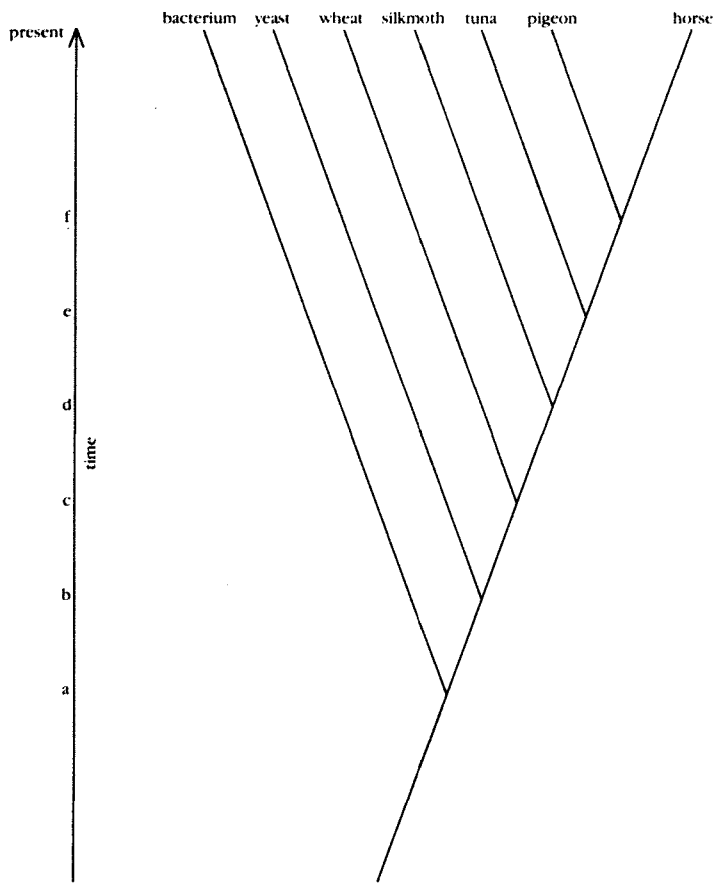

Figure 4
A cladogram generated from the data in Figure 2

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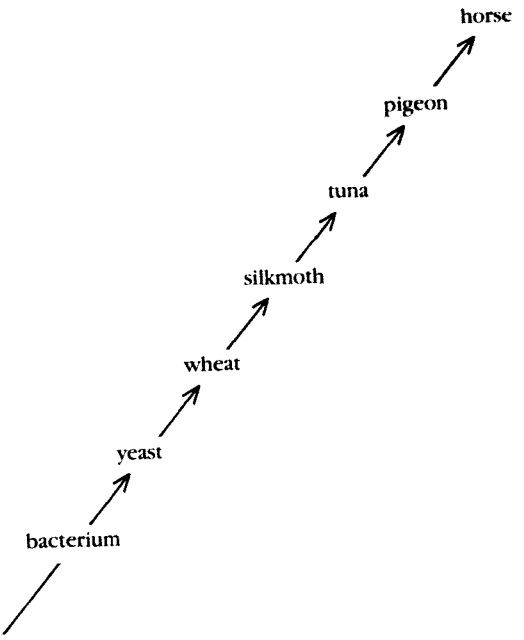

Figure 5
Linear evolution, a widely held but mistaken concept

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Tetrapod Fossil Footprints, Polonium Halos, and the Colorado Plateau

Stephen J. Godfrey

Introduction

In his recent book *Creation's Tiny Mystery*, Robert V. Gentry has reiterated the long-held creationist claim that at least some of the formations within the Colorado Plateau "were deposited within a few months of each other only a few thousand years ago" (1988, p. 53). Gentry goes on to make the following assertions:

> Although the flood itself lasted just a year, long-term geological effects may have lasted for hundreds of years thereafter. For example, while the sedimentary rock formations observed in the Grand Canyon are ascribed to the period of the flood itself the erosional processes that cut through the freshly deposited sediments may well have continued for a number of years after the flood. *In my model the bulk of fossil-bearing sedimentary rocks would have formed during the opening and closing stages of the flood, with lesser amounts being formed during the long period of subsidence and run-off after the flood.* [p. 185; emphasis added]

Such ridiculous claims might easily be ignored, but Gentry purports to support them by citing his analysis of secondary polonium halos in samples of coalified wood from three of the geological periods—Triassic, Jurassic, and Tertiary (Eocene)—represented within the Colorado Plateau (Gentry et al., 1976). Polonium halos are microscopic spheres which form as a result of the alpha decay of polonium 210 atoms which accumulate in radiocenters composed of lead and selenium. Gentry (1988) argues that these halos formed as a result of the infiltration and subsequent decay of a single solution of radioactive uranium. Since the half-life of polonium 210 is only 138 days, secondary polonium halos would have developed in the water-

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Figure 1
The geographical extent of the Colorado Plateau
(redrawn from Colbert, 1983)

soaked wood in less than a year. On this basis, Gentry claims that the vast depth of sedimentary rock forming the Colorado Plateau must have been deposited within a matter of months. As in his earlier *Science* article (Gentry et al., 1976), Gentry *suggests*—he does not provide proof—that the uranium solution which infiltrated the wood was the same in the three samples he studied: "The evidence obtained in these experiments *suggested* a common source for the uranium in all the coalified wood specimens. These data *implied* only one uranium solution had infiltrated the different wood specimens" (Gentry, 1988, p. 57; emphasis added). Furthermore, he does not demonstrate that all the halos developed at the same time. He contends

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that these secondary halos could not have (or were very unlikely to have) developed repeatedly in the wood samples from the different geological periods because an "extraordinarily complex, interrelated series of geological events" are necessary for radioactive uranium to infiltrate wood (1988, p. 56). These requirements are:

> 1) water, 2) uprooted trees as a source of the logs and smaller wood fragments, 3) a rich uranium concentration near the wood, and 4) a compression event occurring after the uranium solution invaded the wood, but prior to its becoming coalified. [1988, p. 56]

(Most of the polonium halos are elliptical, presumably the result of compression of the wood after the formation of the halo.) Gentry's claim that polonium halos within geologically disparate coalified wood are unlikely to have developed repeatedly is based upon the assumption that, since the conditions for the formation of secondary polonium halos "appear" to be remarkably complex, the chances of these conditions occurring many times is unlikely and therefore it is more reasonable to assume that they happened only once. Of the four requirements, only the third—a source of uranium—would appear to limit the wide distribution of secondary polonium halos in fossilized wood. I find no compelling reason to believe that the samples of polonium-halo-bearing coalified wood could not have formed independently at different times. There are many "extraordinarily complex" systems and unlikely events which develop repeatedly in nature. An examination of one such phenomenon—the formation of fossil footprints—will demonstrate the fallacy of Gentry's claims and show that he has ignored, for whatever reasons, a large number of publications on trackways of the Colorado Plateau and their bearing on the length of time and the manner in which the plateau was formed. (Ironically, Gentry charges that it is evolutionary scientists who neglect to consider data supporting the flood origins of formations within the Colorado Plateau [1988, p. 53]).

Tetrapod fossil footprints in the Colorado Plateau

The Colorado Plateau, parts of which lie in Utah, New Mexico, and Arizona as well as Colorado (see FIGURE 1, p. 9), is a vast expanse of primarily sedimentary rock characterized by predominantly horizontal strata ranging in age from the Precambrian to the Cenozoic. This block of the earth's crust has remained remarkably undistorted throughout its history, whereas much of the crust around the plateau has been tilted, buckled, and broken (Chronic, 1983). Good exposure of the relatively undistorted strata has allowed paleontologists and geologists to collect and document the occurrence of tetrapod fossil footprints in rocks which span five geological periods, from the Pennsylvanian to the Cretaceous. No fewer than thirteen formations within the Colorado Plateau are now known to preserve a large

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Table 1

A generalized post-Pennsylvanian stratigraphic column of the Colorado Plateau within which are those formations (**) preserving tetrapod footprints

(compiled from Gilmore, 1927; Lockley, 1986a; McKee, 1982; Billingsley, 1978; and Nations and Stump, 1981)

*** = Coalified wood containing secondary polonium halos recovered from these periods**

(Gentry *et al.*, 1976)

PERIODS	FORMATIONS
Tertiary *	
— — — — — — — — — — — —	Kirtland Fm.
	Fruitland Fm.
	Pictured Cliffs
Cretaceous	Lewis Shale
	** Mesa Verde
	Mancos Shale
— — — — — — — — — — — —	** Dakota Ss.
	** Morrison Fm.
	Bluff-Cow Springs Ss.
Jurassic *	Summerville Fm.
	** Entrada Ss.
	Carmel Fm.
— — — — — — — — — — — —	** Navajo Ss.
	** Kayenta Fm.
	Moenave Fm.
Triassic *	** Wingate Fm.
	** Chinle Fm.
	Shinarump Conglomerate
— — — — — — — — — — — —	** Moenkopi Fm.
	Kaibab Fm.
	Toroweap Fm.
Permian	** Coconino Ss.
	** Hermit Shale
— — — — — — — — — — — —	** Esplanade Ss.
Pennsylvanian	** Wescogame Fm.

and diverse footprint fauna (see Table 1).

It is a little-known fact that many extinct animals are known only from fossil footprints. Remarkable as this may seem, fossil tracks are much more common than the fossilized skeletal remains of extinct animals (Mossman and Sarjeant, 1983). The discovery of a fossil trackway demonstrates that the prints were made by a

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living animal capable of walking across a firm terrain, parts of which were covered by fine-grained and plastic sediments of varying water saturations which could conform to the contours of the animal's foot. Furthermore, this sediment would have to have sufficient body to hold the impression of the foot as it was withdrawn (Cole et al., 1985), and if the sediment were to lithify, the concavity of the impression would provide unequivocal evidence as to whether or not the strata had been inverted. Because it is possible to identify major taxonomic groups—and sometimes extinct species—by the configuration of the lithified impressions (Mossman and Sarjeant, 1983), a study of fossilized prints provides information on the diversity of extinct faunas and how they changed through time (Olsen and Padian, 1986; Haubold, 1986; Olsen and Galton, 1984). Fossil trackways also provide information on such things as stride length, pace and pace angulation, trackway breadth, foot length, distance between the pectoral and pelvic girdles (and therefore some indication of the size of the animal), number of toes (the contour of the print may allow one to determine the number of phalanges per toe, which can help in determining what type of animal left the impression), posture, direction and speed of movement, behavior (whether the animals traveled in herds or singly), and paleopathology (deformities in the foot; Lockley, 1986b). Sometimes even the texture of the integument (skin) is preserved in the sediment. Fossil trackways can also furnish information on former shoreline trends and depositional cycles (Lockley, 1986a, 1987; Lockley et al., 1983; Gillette and Thomas, 1983).

Discussion

In order for fossil footprints to form, a relatively solid surface must exist over which a living animal can walk; trackways are not made by the carcasses of drowned animals. As the Noachian flood was supposed to have killed all terrestrial four-footed critters except those in the ark, creationists *must* explain the presence of fossil trackways at *many* levels within the Colorado Plateau, as well as how they were made while the flood waters were raging. Fossil footprints do not form spontaneously from suspended sediments swirling within flood waters. It is difficult to imagine how delicate footprints could have formed—not to mention how these footprints survived flood waters of creationist proportions.

In order to preserve a recognizable footprint, the sediment over which an animal passes must be fine-grained. If the sediment is not sufficiently fine-grained, a small footprint will not stand out against irregular undulations or deformities in the medium; for example, footprints don't preserve well in coarse sand or gravel, both of which provide poor resolution. The preservation of a footprint is directly proportional to the size of the animal; large and deeply impressed prints are not easily destroyed (Mossman and Sarjeant, 1983). In addition to a fine-grained sediment, the plasticity of the medium is crucial. A sediment with too little or too much water

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will not preserve a recognizable footprint should the medium lithify (Brand, 1979). Subsequent to the formation of the print, it must be covered with additional sediment in order to eliminate the effects of wind, rain, and flowing water which would erase any trace of the impression over time. If the sediment which covers the impression is nearly identical in its composition to that holding the print, then it is unlikely that the print will ever be discovered because the layers will not readily separate to expose the fossil footprint (Mossman and Sarjeant, 1983). Finally, it is essential that the footprint-bearing sediment lithify if the print is ever to survive. Clearly, the quality of a fossil footprint depends upon a number of delicately balanced and interrelated factors. The fact that footprints often vary in the quality of resolution even over a few strides attests to the fortuitious and often precarious interplay of conditions necessary for their preservation. I think most people would agree from their own experience that footprints are ephemeral at best, and the remarkable fact that so many fossil trackways are preserved in the Colorado Plateau certainly demands the attention of creationists.

It is also important to note that fossil footprint species are not randomly distributed throughout the plateau. (The skeletal remains of the fossil footprint-maker may never be found; nevertheless, a footprint kind or "species" name can still be assigned to fossil footprints that exhibit unique morphological features.) Moving vertically from the Pennsylvanian to the Cretaceous, changes occur in the fossil footprint fauna of each formation. Pennsylvanian and Permian formations of the plateau preserve trackways of generally small, four- and five-toed amphibians and early reptiles (Lull, 1918; Gilmore, 1926, 1927; Baird, 1965; McKee, 1982). The footprint fauna changes more conspicuously in the Triassic formations with the advent of a suite of reptile prints, including relatively small three-toed dinosaur footprints (Peabody, 1948; Welles, 1971). Once into the Jurassic formations, the footprint fauna changes again with the addition of new types of dinosaur and other footprints, both large and small (Baird, 1980; Stokes and Madsen, 1979; Lockley, 1986a, 1987). The Cretaceous footprint fauna changes little from that of the preceding Jurassic period in terms of its major taxonomic constituents, but changes in the configuration of the footprints (which mirror anatomical changes) indicate that different species were responsible for making the trackways.

The presence of fossil footprints in the Colorado Plateau provides unequivocal evidence for the sequence in which the formations which make up the plateau were deposited. (Incidentally, fossil footprints do not sort hydrodynamically; the smallest footprints are at the bottom of the plateau, while the largest are at the top.) Pennsylvanian tetrapod trackways (the lowest currently known tetrapod tracks within the rocks of the plateau) were made at a time when the sediments forming those rocks occupied the outermost surface on the earth. Logically, no other younger sediments could have covered Pennsylvanian age sediments or the trackways would never have been made. After these Pennsylvanian tetrapod trackways were made, they were buried by sediment deposited by wind or a body of water (either fresh or

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marine) which covered the land. At a later date, the water receded, exposing the land over which terrestrial animals would once again walk. Then, repeating the cycle, the tracks were buried by wind-borne sediments or water. This same sequence of events occurred many times throughout the Pennsylvanian, Permian, Triassic, Jurassic, and Cretaceous periods, causing a thick sequence of sedimentary rock to accumulate. It is important to remember that thick layers of a variety of sedimentary rocks often separate succeeding trackway horizons; this indicates that these trackways (and the animals which made them) were separated in time by periods required for the intervening sediment (now rock) to be deposited. In other words, the trackways near the top of the plateau could not have been made before those near the bottom, nor could any of the trackways have formed after all the sediments had been laid down. Unlike percolating groundwater and its constituent dissolved minerals (including radioactive uranium), footprints do not infiltrate sediments.

The presence of fossil footprints at different horizons within the Colorado Plateau proves that the conditions for the formation and preservation of footprints occurred many times. On the basis of their configuration, most trackways were made by animals moving along at a "relaxed" pace, not fleeing rising flood waters. The fine-grained nature of the sediment and the quality of preservation proclaim that calm environmental and meteorological conditions were prevailing, not those required for global catastrophe. It would not be unreasonable to assume that the extinct track-makers were surrounded by a thriving biological community, not a denuded landscape awash under millions of tons of sediment-laden flood water.

For these reasons, Gentry's claim that the formation of secondary polonium halos in samples of Triassic, Jurassic, and Tertiary (Eocene) coalified wood supports the hypothesis of the recent formation of the Colorado Plateau is refuted by the presence of footprint impressions at many levels within the plateau. Regardless of when and how the secondary polonium halos were formed, it would have been impossible for all of the footprints to have formed at the same time. Furthermore, fossil footprints are not subject to the same uncertainties that appear to plague the study of polonium halos, such as whether or not the ratio of uranium 238 to lead 206 has been altered by remobilization (uranium addition, lead removal, or both) through groundwater circulation after coalification, and whether or not the ratio is indicative of the formation time of the radiocenter. While the formation of secondary polonium halos remains contested and equivocal, fossil footprints and the trackways that they form are the actual record of an animal's activity at a specific location and time in the past. Gentry asserts that the sedimentary material that formed the rock of the Colorado plateau "could have been deposited both during the time when the waters were rising and again when they were receding" (1988, p. 53). Fossil footprints show this claim to be utter nonsense.

I have limited this discussion to tetrapod footprints of the Colorado Plateau because this is the area from which Gentry and his colleagues obtained their secondary polonium-halo-bearing coalified wood samples (Gentry et al., 1976). I should

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point out that there are many other types of ichnofossils in the Colorado Plateau that I have not considered, such as those made by burrowing invertebrates or scurrying arthropods. Of greater importance than this is the fact that ichnofossils are found worldwide (for a very small sample of the available literature, see Hitchcock, 1858; Frey, 1975; Hantzschel, 1975; Gillette, 1986).

In summary, secondary polonium halos in coalified wood samples from the Colorado Plateau *must* have developed repeatedly because fossil footprints, which are unequivocal in the nature of their origin, occur at many levels within the plateau. These multiple footprint horizons formed repeatedly over great periods of time whenever conditions favorable for their formation and preservation prevailed. Therefore, these fossil footprints (and all trace fossils) refute Gentry's claim that Triassic, Jurassic, and Eocene rocks of the Colorado Plateau "all originated at about the same time, in agreement with the flood-related scenario" (Gentry, 1988, p. 55). If the bulk of the Colorado Plateau was formed during the worldwide Noachian flood, then lithified trackways would occur—if at all—only in the lowermost levels of the plateau. The fact that this is not the case proves that the sedimentary rocks of the plateau are not the result of a single global flood. This is but one example of how these sometimes neglected fossils can be used to refute the spurious claims of a young earth and highlight the egregious incompetence of some creationists. Gentry is welcome to believe that "the Genesis record of creation and the flood is the master key which unlocks all of Earth's geologic history" (1988, p. 185), but this belief is not supported by the evidence and therefore should not be taught as science.

Acknowledgements

I would like to thank B. Redifer for bringing the work of Robert Gentry to my attention. The quotes in this article were excerpted from *Creation's Tiny Mystery* by permission of the author. Furthermore, I am indebted to C. Hall and L. Johnson of the Tyrrell Museum of Palaeontology for providing literature on tetrapod trackways of the Colorado Plateau.

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A Further Examination of the Research of Walter Brown

by Jim Lippard

I am glad that Walter Brown took the time to write a response to my article (Lippard, 1989; Brown, 1989d). I believe that his response provides a good example of his research and debating methods. Brown has failed to address a number of my major criticisms, as I will show below.

The Out-of-Date Book

In Brown's general remarks, he complains that my comments were based upon "an outdated edition (1986) of *The Scientific Case for Creation*." In a later paragraph, he also states, "Since Lippard also gives the wrong publishing date for *In the Beginning* (1987 instead of 1986), I wonder if he actually has the book." An explanation is in order.

When I first began corresponding with Brown, he sent me a version of his book (in January 1987) which I wrongly assumed was a prepublication copy. I noted in my bibliography that this was what I was working from, and the cover

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letter which accompanied it was reprinted in full in my article. This version of the "120 Categories of Evidence" included no mention of Setterfield or the speed-of-light decay. The fifth edition of his book (Brown, 1989c) discusses Setterfield in a section separate from Brown's "categories of evidence" (which now number 127). My remarks would have been accurate if I had simply praised Brown for not listing the speed-of-light decay in his categories of evidence rather than for not listing them in his book at all. On the other hand, I have no excuse for listing the wrong date of publication, since Brown stated it in the accompanying letter. Unfortunately, I mistakenly listed the year of the *letter* instead.

When I originally submitted my article to *Creation/Evolution* in December 1988, I simultaneously sent a copy to Brown. I specifically asked Brown if he had any corrections or suggestions to offer before publication, and he had none—he simply stated that he would respond if his rebuttal would be published by *Creation/Evolution*, unedited and combined with my article with his comments "point-by-point and side-by-side" with mine (Brown, 1989a). He did not inform me that a new edition of his book was available, though he states in his published response that "this latest edition was sent to *Creation/Evolution* when they notified me that they were considering printing Lippard's article." I am surprised that Brown did not have the courtesy to send me a copy as well—but not too surprised.

Brown also notes in his response that yet another edition of his book would be published in September 1989. When I wrote to ask for an advance copy, or at least an update to the version I have, Brown stated that no advance copies would be available and that he was completely sold out of his present edition. I have since purchased a copy of this latest edition.

But, by Brown's own claims, my reliance on the old edition should not have posed a problem. His response accepted only two of my criticisms—both of which he classified as "minor" and claimed to have already corrected independently.

Brown's Debate Contract

Brown says that his "contract" is a mere "statement of agreement" (I won't quibble over terminology) and asks, "What's wrong with that?" My main objection is to the odd credentials requirement. Brown states that I myself, a graduate student in philosophy, would be able to debate him only if I "team up with a scientist, a science professor, or a person with a Ph.D. in a technical field" (Brown, 1989d, p. 36). This requirement allows for someone with a Ph.D. in, say, computer science to debate Brown, while a holder of a master's degree in biology would not make the grade—even though the latter's degree would be more directly applicable to the subject at hand. Moreover, restricting the opposition to holders of doctorates minimizes the number of potential opponents, since such people are less likely

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to have the time or desire to debate Brown, who does not himself hold a degree in a field directly relevant to evolution (his Ph.D. is in mechanical engineering).

The "Categories of Evidence"—Again

One of the criticisms of Brown's book in my original article was that his categorization scheme is bogus. In his rebuttal, Brown not only ignores the main substance of my criticism, he continues to refer to the fact that I have addressed "only parts of a small percentage" of his categories. It is worth taking a closer look at his categorization scheme. Of his 127 categories, twenty-six (20 percent) are alleged dating techniques which produce young ages for the earth and the universe; most of these are quite familiar to readers of this journal. A full thirty-two (another 25 percent) argue for the reality of the biblical flood and Noah's ark (eleven ark sightings, four flood-and-ark plausibility claims, and seventeen allegedly unexplainable geological features which Brown thinks a worldwide flood would explain). I believe that *all* of these categories—amounting to nearly half of Brown's book—may be quickly disposed of and, in fact, have already been soundly refuted in publications to which Brown should have easy access.

Brown's response to this point completely ignores my criticism of his bogus classification scheme which counts individual ark sightings as separate "categories." Instead, he argues against only my objections to certain categories which I claimed did not meet all three of his criteria for inclusion in his list: (*a*) being scientific, (*b*) opposing evolution, and (*c*) supporting creation. He objects to my classification of one point (an argument from design) as "philosophical" rather than "scientific" (though he then brings up an admittedly philosophical argument from his book), defends his category arguing against Lamarckism, and objects to my description of one category as "simply argu[ing] for the possibility of special creation."

On the last point, I accept Brown's criticism regarding category sixteen (formerly category fifteen), which I stated "simply argue[d] for the possibility of special creation" (Lippard, 1989, p. 24). Brown has, as he demonstrates, made the much stronger claim that some evidence actually favors a common designer over a common ancestor. I will not address the claim itself here, except to note that I do not find convergent evolution—similar environmental challenges producing similar solutions in creatures with different underlying structures—to be particularly surprising. Brown should also note that there is strong evidence favoring common ancestry over common design (for example, Max, 1986).

On the other points, I reject Brown's arguments. He maintains that the sonar of marine mammals, the radar of the bat, the flight of the hummingbird, the bombardier beetle, and other examples point to the existence of a designer. This is simply the standard philosophical argument from design. On the other hand,

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within his argument Brown does make the scientific claim that "the many components of each complex system could not have evolved in stages without placing a selective disadvantage on the animal." But he offers no argument in support of this claim, and there are reasons to doubt it—at least in the case of the bombardier beetle (Weber, 1981a, 1981b). Further, many cases of *bad* design can be adduced against his argument—such as python and whale vestigial hips, the panda's thumb, and the kiwi's egg (*see*, for example, Gould, 1980).

As Brown himself admits, his argument about evolution implying the randomness of thoughts is philosophical and therefore does not meet his criteria for being included among the "127 Points." Since philosophy is my area of specialty, I will address this argument shortly.

Regarding my objection to an argument against Lamarckism as a category of evidence against evolution, Brown responded by citing an editorial in the January 12, 1989, issue of *Nature* which discusses several recent papers defending some "neo-Lamarckian" mechanisms in bacteria. Brown sees this as evidence that "modern evolutionists, frustrated at not being able to find some mechanisms for macroevolution, are considering Lamarckian concepts" (Brown, 1989d, pp. 36–37). Thus, Brown sees his category not as attacking a straw man but as a position still being used by evolutionists today. But what Brown's category actually says is: "Acquired characteristics cannot be inherited. For example, the large muscles acquired by a man in a weight lifting program cannot be inherited by his child." This *is* a straw man. No scientist would defend Brown's example or anything like it. And while the neo-Lamarckian mechanism described in *Nature* is quite controversial and under attack by scientists (*see* the papers cited in the editorial), Brown's point offers no evidence against it. This category is not evidence against evolution, as I maintained in my original article.

Random Thoughts

Brown's admittedly philosophical argument is:

> If life is ultimately the result of random chance, then so is thought. Your thoughts—including what you are thinking right now—would, in the final analysis, be a consequence of a long series of accidents. Therefore, your thoughts would have no validity, including your thought that life is a result of chance or natural processes. By destroying the validity of ideas, evolution undercuts even the idea of evolution. [Brown, 1989c, 1989d]

As a first objection, this argument conveniently ignores theistic or directed evolution, in which either the ultimate source of thought, the mechanisms which brought it about, or both are the result of God's plan rather than "random chance." This

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alone is sufficient to eliminate this as an argument against evolution—at best, it is an argument against atheism. Yet, even at this it fails because at least one of its premises is false and its conclusion does not follow from its premises. Evolution does not postulate that thought arises from chance alone but with the help of natural selection, which is not at all a chance process. The mechanisms which themselves produce thought do not do so by random means, though it is quite probable that the brain makes use of some random processes. Hidden within this argument is the unstated premise that any structure created by random processes cannot make valid inferences. This is a highly implausible premise. It seems quite clear that an appropriate structure, *whatever its origin*, is capable of producing valid conclusions from correct premises.

Walter Brown and Barry Setterfield

In my previous article, I noted that Brown was quoted in the Australian creationist journal *Ex Nihilo* as having called Barry Setterfield's work on speed-of-light decay "virtually unassailable." Brown claims that he never made the comment and that, to his knowledge, he has never been quoted in *Ex Nihilo*. He further asks, "Who is the source of Lippard's '*Ex Nihilo*, 1984,' reference?"

A complete citation was given in the bibliography to my article. The quotation was from a page of quotations entitled "Update: On what's being said about Barry Setterfield's work on the speed of light" on page forty-six of *Ex Nihilo*, volume six, number four. The portion relevant to Brown reads:

> DR. WALTER BROWN, MATHEMATICAL PHYSICIST, DIRECTOR, MIDWEST SCIENCE CENTRE, CHICAGO, U.S.A.
> "The theoretical derivation of the same cosec 2 decay function as the computer curve, but this time from electromagnetic theory alone, makes the whole proposition of c decay virtually unassailable."

At about the same time that this issue of *Ex Nihilo* was published, Brown was director of the Midwest Center of the Institute for Creation Research in Wheaton, Illinois—a suburb of Chicago (Schadewald, 1983, p. 29). Thus, there is little question that we are talking about the same Walter Brown. After reading Brown's rebuttal in *Creation/Evolution*, I sent him a letter dated June 19, 1989, in which I enclosed a photocopy of the relevant page from *Ex Nihilo* and suggested that if the quote was misattributed or otherwise in error Brown should notify the journal of that fact. Brown's remarkable response was:

> You suggested that I write *Ex Nihilo* about the incorrect quote. I learned some years ago that I did not have the time to contact and attempt to get

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> retractions every time I am misquoted. Distortions of my statements, which are usually made by evolutionists, are made too frequently. [Brown, 1989b]

I responded to this letter on July 27, asking Brown for elaboration on precisely what was "incorrect" about the quote and offering to write to *Ex Nihilo* on his behalf if he was really too busy to set his fellow creationists straight. I further asked if perhaps he might have made such a statement in one of the "detailed, face-to-face discussions" he said he had with Setterfield in 1984 (Brown, 1989d, p. 39). Brown has failed to respond to this and followup letters sent on August 19, September 14, and November 13 repeating these and other questions.

Recently, however, Ken Smith (1989) informed me that the above quotation from Brown appears in its entirety in *The Velocity of Light and the Age of the Universe*, a technical monograph by Barry Setterfield (1983, p. 172). I have verified this by obtaining a copy of the relevant page. So, the quote predates Brown's 1984 discussions but not his "letter to Setterfield in 1981" (Brown, 1989d, p. 39). Since Brown was in contact with Setterfield, it stands to reason that Brown was aware of this publication, yet apparently he did not complain to Setterfield about being misquoted when he met with him in 1984.

Brown claims that he has "always tried to carefully point out that [Setterfield's work] may turn out to be wrong" (Brown, 1989d, p. 39). Why, if this is so, is there not a word of skepticism to be found in the coverage of Setterfield in his book (Brown, 1989c, pp. 89–92)? Not a single critique of Setterfield is mentioned. It is clear both from his book and his rebuttal that Brown still uncritically supports Setterfield, based upon the results of Brown's own "very time-consuming computer simulation technique" and the fact that "statisticians in various countries have reached similar conclusions" (Brown, 1989d, p. 39). Unfortunately, Brown gives us no citations for his work nor that of these unnamed statisticians. He also criticizes Gerald Aardsma's debunking on the basis of its statistical weighting procedure. Unfortunately, he fails to address a far more significant problem pointed out in Aardsma's article: that Setterfield uses erroneous values for two of the earliest data points—measurements from seventeenth-century data of Roemer and Cassini. This and other problems are dealt with in more detail in yet other debunkings of Setterfield by two of his fellow creationists, in articles in the *Creation Research Society Quarterly* (Aardsma, 1988; Humphreys, 1988). Does Brown's analysis take into consideration corrected values for the Roemer and Cassini points? If not, his alleged result supporting Setterfield is meaningless. Brown has failed to respond to my inquiries about this issue.

It should also be noted, as Aardsma does (1988, p. 39), that if "atomic time" and "dynamic time" are diverging, as Barry Setterfield claims, then the radiocarbon date for the Dead Sea Scrolls of 20 BCE, plus or minus two hundred years, indicates an actual date of about 800 CE, contrary to paleographic and archaeological evidence that strongly supports the radiocarbon date.

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More Speed-of-Light Decay

In his rebuttal in *Creation/Evolution*, Brown goes on to cite more evidence which he claims supports speed-of-light decay. The first is an article by a Soviet cosmologist (Troitskii, 1987), whom Brown says "has concluded, independently of Setterfield, that the velocity of light was ten billion times faster at time zero" (Brown, 1989d, p. 40). Actually, Troitskii does not conclude this; rather, he proposes a model (which he claims is, at least in principle, testable by experiment) that involves a decrease in the speed of light. Troitskii also suggests that events are compressed in the past, so everything went faster—including evolution (Troitskii, 1987, p. 408). This paper reminded me of an Egyptian mathematician's model which maps the universe into a hollow earth and applies the same inversion to the laws of physics (Gardner, 1988, pp. 356–358).

Brown's second reference is to T. C. Van Flandern's claim that the gravitational constant is decreasing (Van Flandern, 1981, 1984). Van Flandern himself carefully words his conclusion to say, "The *tentative* conclusion . . . is that the universal gravitational constant *G seems* to be decreasing with time" (1984, p. 627; emphasis added). And based upon the work of other scientists, this tentative conclusion appears to be incorrect. In the same book in which Van Flandern's article appears, there is a paper that casts doubt on Van Flandern's conclusion by citing others who found no variation in *G* (Vellot, 1984). Brown does not cite this paper. And in another study, Hellings and his colleagues found no variation in *G* within smaller limits than those given by Van Flandern (Hellings et al., 1983). There are difficult problems in measuring *G* (*see* Will, 1988, pp. 160–180, for details along with commentary on Van Flandern), but at present the weight of the evidence does not support Van Flandern. (Ken Smith discusses Barry Setterfield's use of Van Flandern and cites additional criticisms on p. 26 of his unpublished manuscript.)

Finally, Brown claims that quasars are evidence for the nonconstancy of *c* and cites a paper from a British popular electronics magazine that claims radio signals have been sent at speeds faster than light (Pappas and Obolensky, 1988). Regarding the first claim, Brown says, "Several years ago this was thought to be simply a relativistic effect that occurs when the jets are aimed at the earth" (Brown, 1989d, p. 40). According to Tom LeCompte, a physicist at Northwestern University, this is still thought to be the case. In response to Brown's claim that "it is simply too improbable that so many random jets would be moving toward the earth," LeCompte offers the following analogy: "Imagine a forest populated with people carrying flashlights. You might wonder why every flashlight you see is pointing towar 's you. It's only because if it was pointed in another direction, you wouldn't see it" (LeCompte, 1989).

Regarding the faster-than-light radio signals, LeCompte suggested that the result might be explained by crosstalk but said that he would like to see the experi-

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ment repeated. A problem with the faster-than-light conclusions is that it requires abandoning special relativity, while recent experiments at Stanford and CERN have verified SR predictions to one part in one hundred billion. If, as Pappas and Obolensky claim, the speed of light can be exceeded by a factor between two and one hundred, these SR predictions should likewise have been off by a factor between two and one hundred.

Dating Techniques

In Brown's first paragraphs regarding speed-of-light decay, he claims that "most scientific dating techniques indicate that the earth, the solar system, and the universe are young—possibly less than ten thousand years old" (Brown, 1989d, p. 38) and notes that his book lists twenty-four of them (the new edition has twenty-six). And just what are these "scientific dating techniques"? Why, the same old tired, invalid creationist arguments readers of this journal are probably quite familiar with. I will present a few examples to illustrate.

Brown's category seventy-six states:

> The rate at which elements such as copper, gold, tin, lead, silicon, mercury, uranium, and nickel are entering the oceans is very rapid when compared with the small quantities of these elements already in the oceans. There is no known means by which large amounts of these elements can precipitate out of the oceans. Therefore, the oceans must be very much younger than a million years.

Brown is referring to the residence times of these elements, which range from 2,000 years for lead to 560,000 years for gold (Miller, 1984, p. 46). Brown doesn't mention that other elements have much higher and lower residence times—for example, one hundred to two hundred years for aluminum, titanium, and iron, and 2 million to 260 million years for silver, potassium, magnesium, and sodium. This wide diversity of times shows that the use of residence times of various elements in the oceans is not a reliable dating method. Furthermore, Brown's statement, "There is no known means by which large amounts of these elements can precipitate out of the oceans," is simply false. Nickel, for example, may be taken up by ion exchange into clay minerals, substitutes for magnesium in crystal lattices, and forms insoluble hydroxides in alkaline solutions (Dutch, 1982, p. 30). These elements are also absorbed by plants and animals, collect on the ocean floor, and are subducted into the mantle of the earth at various locations (Miller, 1984, p. 47; Strahler, 1987, pp. 144, 149; Van Till et al., 1988, pp. 83–91).

In category ninety-two, Brown claims that the sun is shrinking at a constant rate and that it cannot be more than a million years old. In support, he cites three

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sources (Dunham et al., 1980; Gribbin and Sattaur, 1984; Lubkin, 1979). Unfortunately, two of his sources are out of date and the third supplies evidence that undercuts Brown's claim. From the very beginning, the claim of a shrinking sun was disputed by contrary evidence (for example, LaBonte and Howard, 1981; Parkinson et al., 1980; Shapiro, 1980; Stephenson, 1982). It now appears that the sun oscillates on about an eighty-year cycle (Gilliland, 1981; Parkinson et al., 1980; Parkinson, 1983). It should be noted that several of the authors of one of the papers Brown cites (Dunham et al., 1980) recently concluded that "the solar radius changes are not secular (monotonic and uniform)" and that "the Mercury transit data convincingly disproved the existence of large secular changes in the solar radius" (Sofia et al., 1983, p. 525). More recently, the claim has again been made that the sun was once significantly larger than it is now, specifically during the seventeenth century (Ribes et al., 1987), but this study failed to account for certain systematic instrumental effects which invalidate the claim (O'Dell and Van Helden, 1987). Measurements of the solar radius, contrary to Brown, are not a reliable measurement of the sun's age. (*See* also chapter three of Van Till et al., 1988, pp. 47–65, for a description of creationists' use of the "shrinking sun" claim and the evidence against it, including a note on p. 51 that a 1984 paper by Claus Frohlich and John Eddy reported an *increase* in the solar diameter between 1967 and 1980. The chapter comments specifically on Brown and was originally published in the September 1986 *Journal of the American Scientific Affiliation*, so Brown should have been aware of its existence.)

Some other bogus dating methods Brown lists are the decay of the earth's magnetic field (for the flaws in this method, *see* Dalrymple, 1983; Strahler, 1987, pp. 150–155) and the quantity of interplanetary dust in the solar system and on the earth and moon (for flaws, *see* Dutch, 1982; Strahler, 1987, pp. 143–145; Van Till et al., 1988, pp. 67–82). These account for another five of Brown's categories: seventy-eight, seventy-nine, eighty-two, eighty-nine, and ninety.

Two- to Twenty-Celled Life Forms

Regarding my comments on category eighteen of his book, Brown seems to have missed the point I was trying to make. He argues that neither Mesozoa nor colonial forms of life could be intermediate between unicellular and multicellular life forms. But my point was simply that there *are* forms of life with between two and twenty cells, and some of them are described in the very book Brown cited claiming that there aren't. Two examples were given in my original article. Strictly speaking, Brown's claim in his book that "there are no forms of *animal* life with 2, 3, 4 . . . or even 20 cells" (emphasis added) is correct because his qualifier restricts the claim to the animal kingdom. But, as I noted in my original article, this is misleading because there seems to be no reason to make such a restriction when

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looking at the possible evolutionary origins of multicellular life.

Archaeopteryx

In response to my criticism of Brown's position on *Archaeopteryx*, he states that he "never said that *Archaeopteryx* was a hoax" (Brown, 1989d, p. 42). No, he didn't come right out and say that it was a hoax; he merely implied it by stating that "a strong case can be made that . . . the imprint of the feather was added after the fossils were discovered" (Brown, 1986, p. 23). Brown defends his approval of the hoax hypothesis by asserting that "as of 1986, at least, [Hoyle et al.] were making a good case" (Brown, 1989d, p. 42). Others dispute that they ever had a good case. Halstead (1987) describes it with such comments as "contains demonstrable falsehoods," "absurdity is piled on absurdity," "willful ignorance," "patently ludicrous," and "displays utter contempt for minimal standards of scholarship." Kemp (1986) states that Hoyle and his colleagues "exhibit a staggering ignorance about the nature of fossils and fossilization processes" (*see also* Charig et al., 1986; Gould, 1987; Whybrow, 1986.) But Brown's comment implies that he now no longer thinks the hoax hypothesis is a good one. So has he removed it from his book? No. The hoax hypothesis is still presented in Brown's book, and not a single anti-hoax article is cited (Brown, 1989c, pp. 35–36). Furthermore, the book still maintains that "only two [*Archaeopteryx*] fossils have clearly visible feathers" (Brown, 1989c, p. 35), despite the fact that Brown knows there is now another specimen with clear feather impressions.

Regarding Brown's other argument (that *Protoavis* destroys the status of *Archaeopteryx* as a transitional fossil): while *Protoavis*, if a bird, would probably eliminate *Archaeopteryx* as an ancestor to modern birds, the identification of *Protoavis* as a primitive bird is not yet generally accepted (Gee, 1988).

Brown also refers to "the scarcity, or perhaps complete absence, of transitional forms in the fossil record" (Brown, 1989d, pp. 41–42). Apparently, he is unfamiliar with sizeable lists of transitional forms, such as those in Cuffey (1972).

Human Evolution

I accept Brown's correction to my statement of his position that "fossils of early humans are either apes or modern humans." (His position is that "*alleged* human ancestors were either apes, humans, *or hoaxes.*")

Regarding Charles Oxnard's studies of australopithecines, Brown's response ignores a significant portion of my comment—that these studies did not take into account Donald Johanson's "Lucy" find or any examples of *Australopithecus afarensis*. To Brown's credit, he has changed the latest edition of his book to make

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note of this fact in a single sentence of a footnote (Brown, 1989c, p. 39). However, because his main text is still quite misleading and because the footnotes and references are all listed together in one large section following his "categories of evidence," most of his readers are likely to miss the clarification.

Citing Oxnard in support of the claim that no australopithecines are in the human lineage is extremely misleading, because Oxnard himself states that *Australopithecus afarensis* is probably in the human lineage. He states that, if scientists will use the term *australopithecine* (which has indeed become the case) for finds at East Turkana, Laetoli, and the Afar Valley (for example, *Australopithecus afarensis* in the latter two locations), "the new usage of the term will be for these other fossils labeled 'Homo?' in figure 18" (Oxnard, 1979, p. 274). But Brown, who cites this very paper, makes the misleading maneuver of summarizing Oxnard by stating, "Detailed computer studies of the Australopithecines have conclusively shown that they are not intermediate between man and living apes. . . . The Australopithecines are a type of extinct ape" (Brown, 1986, p. 4; 1989c, p. 6; 1989d, p. 43; the new version of the book removes the word *conclusively*).

Brown should also be aware that, contrary to the view I expressed in my original article, Oxnard says that *Ramapithecus* may well be a human ancestor, based upon the same sorts of studies that led him to reject a role of some australopithecines in the human lineage (Wu and Oxnard, 1983).

Brown goes on to argue against the bipedalism of *Australopithecus afarensis* in two ways: first, he notes that "Lucy's alleged knee joint implies that she *could* walk upright. That does not mean she did"; and, second, he claims that Donald Johanson, Lucy's discoverer, made "quite an admission" when he stated that he did not find the knee joint anywhere near the rest of the skeleton. As part of this second argument, Brown also maintains that "Johanson needs to clarify or deny this in writing. None of his published writings do."

I'll take up the latter point first. As Donald Johanson wrote to me in 1989, "If Mr. Brown would use his library card, he would be able to read in *Lucy: The Beginnings of Humankind*, published in 1981, details of these two discoveries; and, in the *American Journal of Physical Anthropology*, April 1982, Vol. 57, no. 4, he would be able to see a complete bibliography of all publications up to that point, concerning the stratigraphic positions and the evolutionary interpretations of the discoveries." Brown should read Johanson's writings before making sweeping claims about what they do or don't say. The time and location of discovery of the first knee joint and of Lucy are quite clearly described in chapters seven and eight of Johanson and Edley's 1981 book. The knee joint, which Johanson discovered during his first expedition to Hadar in November 1973, *has never been claimed as that of Lucy.* Tom Willis (1987), who appears to have been the first to report Johanson's alleged misinterpretation of the 1973 knee joint, claims that when Johanson was asked, "Why are you so sure [the knee joint] belonged to Lucy?" he answered, "Anatomical similarity." I suspect that the question Johan-

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son was really answering (or thought he was answering) was how he knew the knee joint belonged to the same *species* as Lucy. On the other hand, enough of the proximal tibia and distal femur were preserved in Lucy to indicate that the anatomy of the knee joint region was identical to this earlier discovery. Furthermore, another *Australopithecus afarensis* knee joint was discovered at the "First Family" locality at Hadar, situated stratigraphically between Lucy and the original find. All of this is plainly described in the book. Unfortunately, Brown's erroneous implication of wrongdoings by Johanson appears to have been added to the creationist arsenal (for example, Morris, 1989). The same argument was put forth by Michael Girouard of the Institute for Creation Research at a conference in Tucson on December 1, 1989. I informed Girouard that the argument was mistaken and supplied him with a copy of my letter from Johanson.

In response to the first point, there is more evidence than a knee joint supporting bipedalism in *Australopithecus afarensis;* the top end of a thigh bone found by Dr. Tim White and Professor Desmond Clark (Johanson, 1989) and the footprints at Laetoli are but two other examples.

Brown has failed to respond to much of my criticism on the subject of Lucy's bipedality. He cites two articles in support of a claim implying that Lucy swung from trees *instead* of walking upright, but *both* articles *explicitly state* that she was bipedal. His claim that Lucy did not walk upright "in a human manner" is likewise misleading, as even Jungers allows for bipedality. At a meeting on the subject of Lucy's bipedality which occurred subsequent to the articles Brown cites (and which included their authors), all participants agreed that "fossils and footprints indicate that by 3.5 million years ago our ancestors walked on two legs rather than four" (Lewin, 1983, p. 700). Further, the authors of the articles Brown draws on for support agreed that the bipedal footprints at Laetoli were, or could have been, formed by *Australopithecus afarensis* (Lewin, 1983, p. 702).

Brown's rebuttal excerpts the section on Lucy from the more up-to-date version of his book, and it continues to make the same misleading statements (Brown, 1989c, pp. 5-6; 1989d, p. 43). Brown tries to use the controversy over the nature of Lucy's bipedality to show that Lucy was not bipedal at all and not a human ancestor—positions that those on all sides of the controversy would not endorse.

Brown quotes from William Fix to argue that skull ER 1470, even if dated at 1.8 million years, eliminates *Australopithecus africanus* as a human ancestor. But the latest evidence puts *Australopithecus africanus* at 2.8 to 2.2 million years ago (Delson, 1987, p. 654), and most (but not all) anthropologists put it in the human lineage (Simons, 1989, p. 1344).

Regarding my criticism of his descriptions of Java Man, Brown has no response. Regarding Peking Man, Brown cites Boule and Vallois (1957), claiming that they "at least acknowledge the possibility that Peking 'men' were just apes that were hunted by true man" (Brown, 1989d, p. 45). It is true that they acknowledge something to that effect, but not that Peking Man was *just* an ape; instead,

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they explicitly state, "Morphologically, there is not the slightest doubt [as to the place of *Pithecanthropus* and *Sinanthropus* in the hierarchy of the primates]. *Sinanthropus* confirms and completes the proof that these are creatures with physical characters intermediate between the group of Anthropoid Apes and the group of Hominians" (Boule and Vallois, 1957, p. 142). They also state that "those who believe in the simian character of *Pithecanthropus* really look upon it as an Ape superior to all living Apes, while those who believe in its human character regard it as inferior to all known Men, living or fossil" (p. 124).

If Brown really thinks that the evidence on Peking Man favors the creationist view, I suggest that he read C. Loring Brace's 1986 article on the pithecanthropines.

Out-of-Order Fossils and Artifacts

I am glad that Brown has removed *Oreopithecus* and the Moab skeleton from his book. Unfortunately, he goes on to state that he still thinks the evidence is in favor of the Castenedolo and Calaveras being genuine "out-of-order" skeletons. Brown notes that Sir Arthur Keith, describing the Castenedolo skeletons, concludes without argument that they were intrusively buried. Keith writes that Castenedolo "may be dismissed; the unfossilized condition of the remains and other circumstances make us certain we are here dealing with intruded burials" (1925, p. 340). Unfortunately, Keith does not say what these "other circumstances" are. However, another source which Brown has already cited answers the question. In Boule and Vallois's *Fossil Men*, some of these circumstances are described: the skeletons were found in a marine deposit, and all fossils in the deposit—with the exception of the skeletons—were scattered and impregnated with salt (1957, pp. 106–107). Ernest Conrad's article on out-of-order fossils further points out that collagen analysis and radiocarbon tests show the skeletons to be of recent origin—Holocene, not Pliocene (1982, p. 16).

Regarding the Calaveras skull, I am pleased to note that Brown's new book at least mentions the hoax hypothesis and cites some articles defending it (Brown, 1989c, p. 40). Brown should note that in addition to the evidence I previously described, Conrad points out that flourine analysis on the skull also indicates a recent origin (1982, pp. 17–18). Furthermore, R. E. Taylor, director of the Radiocarbon Laboratory at the University of California at Riverside, has informed me that radiocarbon dating shows the age of a bone associated with the Calaveras skull to be 740 (plus or minus 210) years old (Taylor, 1989; publication forthcoming). Dr. Taylor also brought to my attention a recent article giving an overview of the skull's history (Dexter, 1986).

Brown cites William Fix in support of the claim that the Swanscombe, Steinheim, and Fontechevade skulls are evidence that "modern-type humans" were in existence long before Neandertal. There is some controversy over the place of

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Neandertal in evolution. Some recent reinterpretations of South African fossils, new dates on Middle East remains, and genetic studies on mitochondrial DNA lend credence to the possibility of Neandertal being a separate species (*Homo neanderthalensis*) not ancestral to modern *Homo sapiens*, suggesting that Fix is correct (Zegura, 1989). Is this really Brown's position? Most creationists insist that Neandertal was a fully modern human (for example, Gish, 1985, p. 204; Morris, 1985, pp. 175–176). This evidence suggests, quite to the contrary, that Neandertal was a species separate from modern humans.

On the other hand, the Steinheim fossil, while apparently more modern than Neandertal in some respects, looks less advanced in others. Swanscombe and Steinheim, which predate Neandertal, are still considered by many to be intermediate between *Homo erectus* and *Homo sapiens*. The two Fontechevade skull fragments, which do not predate Neandertal but are contemporary with early Neandertal, are significant because one of them (Fontechevade I) is a forehead fragment lacking a brow ridge. Unfortunately, the fragment is of the skull of an immature individual of perhaps ten years old or less, and Neandertal children did not always possess brow ridges. A study of the other fragment, Fontechevade II, found many features similar to Neandertal and also to the earlier Steinheim (*see* Jurmain, Nelson, and Turnbaugh, 1987, pp. 414–417 and 424–425).

Regarding Carl Baugh's hammer, I am happy to say that Dr. R. E. Taylor of the Riverside Radiocarbon Laboratory of the University of California has offered to date the hammer and negotiations with Baugh are presently underway (with Brown as his adviser).

Noah's Ark

Brown's response to my criticisms of his Noah's ark categories is by far the weakest part of his rebuttal. He does not even attempt a defense. The fact is that the total verifiable evidence for the existence of Noah's ark in Brown's eleven "categories" of sightings comes to nil. I pointed out that one story in particular (category 101) originated as an April Fool's joke, yet Brown still lists it in his book without critical comment (Brown, 1989c, p. 20).

According to Brown, "Some writers that Lippard referenced have learned from Ark hunters which stories are false. These are the accounts that they enjoy attacking. Another tactic of theirs is to show contradictions between the false reports and those that appear very credible" (Brown, 1989d, p. 47). In my article, I pointed out that *all* versions of the story Brown lists in category ninety-four contradict other stories that he also lists. Furthermore, nearly every story of Brown's is debunked by the sources I cited. Brown says, "I believe there is archaeological evidence that Noah's ark exists" (Brown, 1989d, p. 47). What is this evidence?

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Conclusion

Brown concludes in his response that I have "taken almost three years to search out what [I feel] are the weakest of the 120 categories of evidence." As I noted in my original article, the points I addressed therein were simply a selection intended to give the flavor of Brown's work. There was no attempt to find the "weakest" of his points. Indeed, if Brown considers these points to be the weakest perhaps he would accept the following counterproposal to his perennial "written debate" challenge: why doesn't he choose what he considers to be one of the strongest of his points to be debated in detail?

Brown concludes his rebuttal with the hope that he and I can agree that "scientific creationism" should be matched up to evolution in the classroom. I have no objection to all of the scientific evidence being discussed, but I strenuously object to the inclusion in the curriculum of unfounded arguments and misleading claims such as those I have pointed out in Brown's work. I hope that Arizona's students will not be subjected to such creationist distortion as a result of Walter Brown's position on the state Board of Education's Essential Skills Committee for Science (Lessner, 1989).

Brown likes to ask rhetorical questions, such as "Shouldn't we teach all the evidence?" (Brown, 1989d, p. 45) and "Why, then, should one side of this question be suppressed?" (p. 46). But as I have pointed out, Brown's own book repeatedly ignores contrary evidence and misleads his readers. If Brown is sincere about wanting the evidence to be presented fairly and completely, he should begin by practicing what he preaches.

Acknowledgements

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A Second Response to Jim Lippard

Walter T. Brown, Jr.

Jim Lippard begins both of his articles (1989, 1990) dramatically by claiming that he will expose my poor research techniques. Most readers, especially evolutionists looking for an expose, naturally expect to read about misquotations, bias, false statements, unsupported conclusions, scientific misunderstanding, and ignored or distorted evidence. However, a careful reader of these last two issues of *Creation/Evolution* will find that it is Lippard who falls short in these areas.

The Out-of-Date Book

Lippard: "When I first began corresponding with Brown, he sent me a version of his book (in January 1987) which I wrongly assumed was a prepublication copy. . . . I am surprised that Brown did not have the courtesy to send me a copy [of his revision] as well—but not too surprised."

Lippard is still confused. I never sent him the book he still claims he critiqued. What I sent him was one reproduced section from the 1986 edition of *In the Beginning*, a section entitled "The Scientific Case for Creation." In January 1989, that section was revised, expanded, and reprinted as a separate booklet (Brown, 1989a). A month later, *Creation/Evolution* sent me Lippard's critique, indicating that the journal would publish it. I wrote to the editor, pointed out some of Lippard's errors, mentioned the new booklet, and enclosed a copy, expecting that *Creation/Evolution* would not publish Lippard's article in light of the errors it contained. I said that if *Creation/Evolution* did publish it, I would like to respond in the same issue. To my surprise, the editor wrote back and said he would still publish Lippard's article and I had two weeks to submit an article if I wished. I complied.

Dr. Brown is the director of the Center for Scientific Creation in Phoenix, Arizona, and has a Ph.D. from the Massachusetts Institute of Technology.

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It is true that I have not responded to most of Lippard's letters demanding answers and explanations. Had I felt that he was open-minded and had scientific competence in any aspect of the origins issue, I would have tried to find the time to respond. However, since the editors of *Creation/Evolution* are willing to publish Lippard's views, I am again happy to respond.

The 127 Categories of Evidence

In Lippard's first article, he casually dismissed sixteen (now seventeen) feature on the earth that support a worldwide flood (1989, pp. 32–33). He said that he had talked to a graduate student who assured him that a flood explanation is unnecessary. In the reference portion of the material I sent to Lippard, I offered to provide an article I had written supporting my claim. Lippard never asked for it. That article, entitled "The Fountains of the Great Deep," explained the many problems with the standard explanations for those seventeen features and how they all appear to be a consequence of a cataclysmic, global flood. I have lectured on this hydroplate theory to over thirty thousand people, including many geologists. From their responses, it is clear that most geology professors (and especially their students) are unfamiliar with most of the problems with these standard explanations. The new edition of *In the Beginning* (Brown, 1989c) describes the hydroplate theory in a twenty-six-page section entitled "The Fountains of the Great Deep." Now that Lippard finally has that section, why has he not commented on it? If he or the student advising him has trouble understanding some of the mechanical aspects of the flood, I suggest they consult a mechanical engineer.

Lippard similarly dismisses or ignores the bulk of the book and specifically addresses only a very small fraction of its substance. If you read it yourself, I think you will agree.

Lippard: "Brown should also note that there is strong evidence favoring common ancestry over common design (for example, Max, 1986)."

Notice how many times Lippard fails to explain his rationale or to describe the "strong evidence" favoring his position. Too often, he baldly asserts that it exists, cites a reference, and skips on to the next point. For me to respond properly in each instance would require more space than I am allowed. I will deal with this particular example in detail as an illustration and merely call attention to some of the others. I must first explain what Max said, and then I will show what is wrong with it.

Edward Max understands what most evolutionists do not: similarity between different forms of life does not necessarily imply evolution. It may imply a common designer. Therefore, Max's argument rests on the following analogy. Copy-

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righted material often contains errors. Anyone who copies such material will copy the errors also. Max then says that, if it can be shown in a court of law "that it was inconceivable that the same errors could have been made independently," then one set of errors was copied from the other and the copyright was violated. However, it must also be shown that both sets of errors did not have a common cause, such as an adverse event or some agent. Because of this second criteria, map makers, for example, add *unusual* intentional errors to their maps to protect their copyrights. Using this analogy but failing to consider or deal with this second criteria, Max argues that genetic errors called *pseudogenes* must have been copied and thus support the evolution explanation.

Segments of apparently functionless DNA and RNA are called *pseudogenes*. Max correctly points out that a specific pseudogene is found in gorillas and humans but not in chimpanzees. Max incorrectly concludes that both humans and gorillas must have received the pseudogene from a very close common ancestor. Since most evolutionists believe that humans are more closely related to chimpanzees than gorillas, Max will have trouble selling his argument even to evolutionists. Throwing out the alleged "human-chimp" connection would require evolutionists to retract too many similarity arguments that have propped up the theory of evolution since Darwin.

Max's case is far from complete. He hasn't identified all known pseudogenes nor shown which organisms do and do not have them. Until he has done so, we cannot compare that data with the macroevolution hypothesis. I have done some similar work with amino acid sequences in the protein cytochrome *C* (rather than pseudogenes) and have followed the work of others who have examined nucleotide sequences. I strongly suspect that such cross-comparisons will continue to contradict the evolution hypothesis as I have shown (Brown, 1989b, pp. 7 and 8).

Let's assume, as Max does, that pseudogenes are functionless. Pseudogenes would then show a degeneration of genetic information. How then did the immense amount of coded, genetic information arise? In our experience, codes are produced only by intelligence, not natural processes or chance. A code is a set of rules for converting information from one useful form to another. Examples include the Morse Code and Braille. The genetic material that controls the physical processes of life is coded information. It also is accompanied by elaborate transmission and duplication systems, without which the genetic material would be useless and life would cease. Therefore, doesn't it seem reasonable that the genetic code, the accompanying transmission and duplication systems, and all living organisms were produced by a very high level of intelligence using nonnatural (or supernatural) processes?

Likewise, no natural process has ever been observed to produce a program. A program is a planned sequence of steps to accomplish some goal. Computer programs are common examples. The information stored in the genetic material of all living things is an extremely complex program. Since programs are not produced by chance or natural processes, the most probable conclusion is that some

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intelligent, supernatural source developed these programs.

Nor does information arise spontaneously, at least in isolated, nontrivial systems. Natural processes, without exception, destroy information. Only outside intelligence can increase the information content of an isolated system. If anyone disagrees, please give me an example. Elsewhere, I have also shown how these observations allow one to infer that macroevolution cannot occur and that a "big bang" could not have preceded life (1989c, p. 3).

Lippard: "[Brown] maintains that the sonar of marine mammals, the radar of the bat, the flight of the hummingbird, the bombardier beetle, and other examples point to the existence of a designer. This is simply the standard philosophical argument from design."

The design in living systems is not just a philosophical argument, as Lippard maintains. My contention—that technologies (such as radar and powered flight) require intelligence and a designer—has its basis in human experience. Lippard's contention—that complex technologies can come from natural processes—has no basis in experience. Which one is more philosophic?

Just the *known* genetic information which defines each subcomponent of each living system is vast enough to quantitatively rule out macroevolution. Few realize that there must be even more information that has not yet been discovered. Each gene codes for a protein. Chromosomes code for many proteins. Where is the information that directs *how* the proteins relate to each other and *when* they are produced? We know which proteins comprise an eye or a brain, but simply mixing the right amounts of these proteins together will not produce eyes and brains. The information that puts those proteins together must be very complex. For example, skill is required to produce the materials that constitute the Empire State Building: various types of wire, glass, concrete, steel, and so forth. However, additional skill is required to select the right materials, bring them to the building site at the right time, and properly assemble them. I suspect that the amount of undiscovered genetic information is of the same order of magnitude as the known information. Do evolutionists understand these magnitudes? If so, why do they ignore the obvious intelligence and design that information, codes, and programs require?

How could the immune system of all animals and some plants have evolved? Each immune system can recognize bacteria, viruses, and toxins that invade the body. Each system can quickly mobilize just the right type of defenders to search out and destroy these invaders. Each system has a memory and learns from each invasion attempt.

If the extensive instructions that direct an animal or plant's immune system were not already programmed into the organism's genetic system when it first appeared on the earth, the first of thousands of potential infections would undoubtedly have destroyed the organism. This would have nullified any rare genetic improve-

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ments that might have accumulated. In other words, the large amount of genetic information governing the immune system could not have started to accumulate in a slow, evolutionary sense. Obviously, for all the organism to have survived, this information must have all been there from the beginning. But that's creation, isn't it?

Lippard: "Further, many cases of bad *design can be adduced against his argument—such as python and whale vestigial hips, the panda's thumb, and the kiwi's egg. . . ."*

Degeneration occurs. What Lippard fails to address is the complexity that must have been there before a design could degenerate and still survive. Keep in mind also that components such as "the panda's thumb," which may not be optimal for the panda, may be optimal for some larger ecological system.

Random Thoughts

Lippard acknowledges that the existence of valid human thought opposes the idea of atheistic evolution and could support the existence of God and that chance alone could never produce valid human thought. Upon this, we agree. However, Lippard believes that chance—if accompanied by natural selection—can produce valid thoughts. I disagree. According to evolutionism, everything ultimately gets back to what can best be described as chance. For example, imagine the first mammal that supposedly evolved. Suppose further that it just barely escaped from the jaws of a reptile. Natural selection was at work, but chance entered in many ways: which sperm united with which egg to produce that mammal, which way the wind blew when the mammal first detected the reptile, what foods the mammal and reptile ate just before their encounter, and thousands of other chance events. Each was a consequence of other nearly random events. If we go back far enough, certainly if we go back to the "big bang," the evolutionist should agree that for all practical purposes, chance events become the input for everything, including natural selection and thought. But if thoughts are ultimately derived from chance events, then how can an evolutionist conclude that his thoughts are valid—especially his thought that evolution happened? If your brain evolved, then it thinks the way it does not because its ideas are true but because they have survival value. Rationality is qualitatively different from either chance or necessity.

Lippard: "Hidden within [Brown's thought] argument is the unstated premise that any structure created by random processes cannot make valid inferences. This is a highly implausible premise."

If Lippard thinks that a structure created by random processes can make valid infer-

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ences, why doesn't he show us one? In the field of information theory, that would be comparable to building a perpetual motion machine.

Lippard: "It seems quite clear that an appropriate structure, whatever its origin, *is capable of producing valid conclusions from correct premises."*

I have said that valid thoughts cannot be produced by chance and natural processes. Lippard says that "valid conclusions" can be produced by "an appropriate structure" having "correct premises." Lippard is obfuscating.

Lippard, who claims that my thought argument is only philosophical, conveniently leaves out the second paragraph from the latest edition of the book:

> Authorities on the mind frequently state that humans use only a small fraction of their mental abilities. If so, how could such unused abilities have evolved? Certainly not by natural selection, since those capabilities are not used. (Brown, 1989c, p. 9)

There is nothing philosophical about that.

Walter Brown and Barry Setterfield

In his first article, Lippard accused me of making a statement which I never made and never would have made. Lippard acknowledged that he was using an anonymous source from the creationist publication *Ex Nihilo.* Over my objection, the editor of *Creation/Evolution* twice replaced the word *anonymous* with the words *Ex Nihilo,* presumably to fit the format of *Creation/Evolution*. Not only did Lippard know he was using an anonymous source, he also used an indirect quotation (hearsay). Either would have suggested to a careful researcher that the quote might be spurious, which it certainly was.

Minor errors in the *Ex Nihilo* statement should also have raised the suspicions of a careful researcher: my organization's name was given incorrectly; I am not a "mathematical physicist"; and the mathematical function whose derivation I supposedly accepted was printed in a way that made no sense. Ironically, the statement that Lippard attributes to me says that I agreed with a certain theoretical derivation. I never did. In fact, in 1986 I told Setterfield that it was wrong. Shortly afterward, he retracted it.

Lippard: "Recently, however, Ken Smith (1989) informed me that the above quotation from Brown appears in its entirety in The Velocity of Light and the Age of the Universe, *a technical monograph by Barry Setterfield (1983, p. 172). I have verified this by obtaining a copy of the relevant page. So, the quote predates Brown's*

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1984 discussions but not his "letter to Setterfield in 1981." . . . Since Brown was in contact with Setterfield, it stands to reason that Brown was aware of this publication yet apparently he did not complain to Setterfield about being misquoted when he met with him in 1984."

Wrong. No statement of mine is in Setterfield's monograph, as originally published in 1983. Page 172 of that printing contains only an *Ex Nihilo* advertisement. Someone must have "doctored" Lippard's copy after 1983. Lippard should quit trying to shift the blame and admit that he reached premature and false conclusions by quoting an anonymous, hearsay statement that contained many other errors as well.

Lippard: "Brown claims that he has 'always tried to carefully point out that [Setterfield's work] may turn out to be wrong.' . . . Why, if this is so, is there not a word of skepticism to be found in the coverage of Setterfield in his book . . . ?"

Only three pages deal with the possible decay in the speed of light: its history, relevance, new and surprising data, past arguments, and the work of many people, including Setterfield. Anyone's scientific work may turn out to be wrong. Certainty doesn't exist in science. For example, I stated, "If either Setterfield or Troitskii's reasoning is correct . . ." (1989c, p. 90) and "If Setterfield is right . . ." (1989c, p. 91). Tentativeness is there.

Lippard: "Unfortunately, Brown gives us no citations for his work nor that of these unnamed statisticians."

What good would citations do? None of this is published. It would be a pound of very dull reading, and few would understand the statistics. David Merkel of Aston, Pennsylvania, a statistician for an insurance company, has written separate computer programs and verified my results. If Lippard can find a competent statistician who will do the same, I will provide that person with enough information to check my results as well.

Lippard: "[Brown] also criticizes Gerald Aardsma's debunking on the basis of its statistical weighting procedure. . . . other problems are dealt with in more detail in yet other debunkings of Setterfield by two of his fellow creationists, in articles in the Creation Research Society Quarterly. . . ."

Setterfield has documented the more serious errors of Aardsma and others and has adequately addressed their relevant criticisms (1988).

Lippard: "Unfortunately, [Brown] fails to address a far more significant problem pointed out in Aardsma's article: that Setterfield uses erroneous values for two of

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the earliest data points—measurements from seventeenth-century data of Roemer and Cassini. . . . Does Brown's analysis take into consideration corrected values for the Roemer and Cassini points? If not, his alleged result supporting Setterfield is meaningless.''

An error in only two of Setterfield's 164 data points would not be fatal to his case, even the two earliest measurements. Furthermore, the jury is still out on those two data points. The more important of the two is Roemer's data of 1675. Goldstein (1973) did the most detailed study of it. However, in 1985 Lew Mammel (1983a) of Bell Laboratories pointed out "a gross conceptual blunder" that invalidated Goldstein's study. Goldstein privately acknowledged this error to me shortly thereafter, promised an official retraction in *The Astronomical Journal*, and promised to send me an early copy. He has done neither. Mammel also identified a second error, which he claims shows that the speed of light was 8 percent greater in 1675 than it is today (1983b). Goldstein, however, disagrees. I will not be sure where the truth lies until this complex study is redone by a neutral third party. (Any takers?) Lippard is aware of none of this. In my analysis, I used a value of c = 292,000 +/− 18,000 km/sec, the most pessimistic value of c as far as *cDK* is concerned. Setterfield's thesis would only have been seriously damaged if the statistical analyses of Aardsma and the other authors mentioned had been correct. They weren't. Setterfield has answered Lippard's comment concerning radiocarbon dating (1988, p. 193).

More Speed-of-Light Decay (cDK)

Troitskii. Lippard doesn't understand Troitskii's paper. Troitskii shows that a ten-billion-fold decrease in the velocity of light would explain many cosmological observations, including the two observations that led to the big bang theory: the redshift of distant starlight and the extremely homogenous cosmic background radiation (CBR). If the CBR is the cold residue of a big bang, its extreme uniformity would mean that there were not early concentrations of matter that would have caused significant gravitational contractions. (A recently launched spacecraft, the *Cosmic Background Explorer*, is now showing even more uniformity in the CBR than expected.) Yet these pockets—or clumpings—are necessary for evolutionist theories on the origin of planets, stars, galaxies, galaxy clusters, and superclusters. If these various bodies didn't evolve, then organic evolution could not begin. On the other hand, if the CBR is not a remnant of the big bang, then a major argument for the big bang disappears. With the announcement this past year of the discovery of the Great Wall (the most massive concentration of matter known in the universe), the ideas of a big bang and of massive gravitational clumpings become ludicrous—so much so, that they are essentially dead. What will evolutionists put in their place? How about creation? (For more on these subjects, *see* Brown, 1989c, pp. 12–13).

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Since the big bang doesn't seem to explain the CBR, why not consider Troitskii's, or even Setterfield's, suggestion that *cDK* has occurred? I admit that it's radical and it may turn out to be wrong, but let's consider it.

Incidentally, very few evolutionists realize that some galaxies and quasars have recently been discovered at what are apparently immense distances. They are so far away that there has not been enough time since the hypothetical big bang for them to evolve and their light to travel to the earth. There are three ways to explain these observations: (1) reject the big bang, (2) adopt *cDK*, or (3) accept creation. Take your pick. (For more details, *see* Brown, 1989c, pp. 92–93.)

Van Flandern. Lippard missed the point I made concerning Van Flandern's work. Without realizing it, Lippard argues for *cDK*.

First of all, Van Flandern states that his measurements show atomic clocks and astronomic clocks do not keep the same time. Lippard does not contest this, nor am I aware of anyone who has. Second, Van Flandern states that he knows of only two explanations for this phenomenon: either the gravitational constant is changing or atomic frequencies and the velocity of light are decreasing. Lippard wisely does not contest this, either. Third, Van Flandern states that he tentatively favors a change in the gravitational constant. Lippard jumps in here, disagrees, and cites several authorities. Fine. I gave other reasons as well (1989c, p. 90). But if the gravitational constant is truly constant, then it follows that the speed of light appears to be decreasing.

Lippard: "(Ken Smith discusses Barry Setterfield's use of Van Flandern and cites additional criticisms on p. 26 of his unpublished manuscript.)"

Again, Lippard alludes to something but doesn't explain it. Since it is not published, what can I or anyone say?

Superluminals. Lippard has learned from Professor LeCompte what he thinks is a way to explain superluminals without accepting *cDK*. LeCompte is referring to what is called the *Blunderbuss* model. Although LeCompte's flashlight analogy may sound plausible to the uninformed, it has few supporters because such a model cannot reproduce the narrow core-jet structures of superluminals. In other words, relativistic effects would not produce beams that looked like LeCompte's flashlight beams.

Faster than light.

Lippard: "A problem with the faster-than-light conclusion is that it requires abandoning special relativity. . . ."

No, it doesn't. Lippard is being an alarmist. The faster-than-light experimental results for both electrical and radio signals *under special conditions* do not require abandoning special relativity any more than special relativity required abandoning

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Newtonian dynamics. Only modifications to our current understanding of special relativity may be required if further experiments support these surprising phenomena. Please notice that no contention of mine rests on these experimental results—only the contention that the light from distant stars cannot reach the earth in less than ten thousand years. It should also be noted that the faster-than-light results for the electrical experiments can be derived from Maxwell's equations, which govern electromagnetic phenomena.

Dating Techniques

Heavy elements in the oceans.

Lippard: "Brown is referring to the residence times of these elements, which range from 2,000 years for lead to 560,000 years for gold. . . . Brown doesn't mention that other elements have much higher and lower residence times—for example, one hundred to two hundred years for aluminum, titanium, and iron, and 2 million to 260 million years for silver, potassium, magnesium, and sodium. This wide diversity of times shows that the use of residence times of various elements in the oceans is not a reliable dating method."

Let's first understand what is meant by a "residence time." If the initial quantity of a substance in a well-mixed reservoir is Q_o and the present quantity is Q, and if the average rate of input and output are R_i and R_o respectively, then the average time that substance *resides* in the reservoir is called its *residence time.* However, if the substance (such as mercury or tin) never leaves the reservoir ($R_o = 0$), as I will maintain, the reservoir is called a *sink*. The substance will have no average residing time; it will steadily grow and become a measure of the sink's age. The maximum age would correspond to its age if $Q_o = 0$. This is illustrated in Figure 1 (p. 44).

Unfortunately, we don't know each metal's average input rate into the oceans since the oceans began; we only know the present input rate. If we assume that the present rate equals the average rate, large errors could occur. Actually, all scientific dating techniques, including the majority that suggest a young earth, make similar assumptions. So, even though many people think that "scientists know the age of the earth," there is considerable uncertainty in all dating techniques. However, since we are trying to distinguish between ages that are almost six orders of magnitude apart (billions of years versus less than ten thousand years), many different dating techniques should shed some light—especially if we don't ignore these key assumptions.

Notice that *maximum age* and *residence time* are computed in the same way. However, the models are quite different. Lippard "sloughed over" this critical dif-

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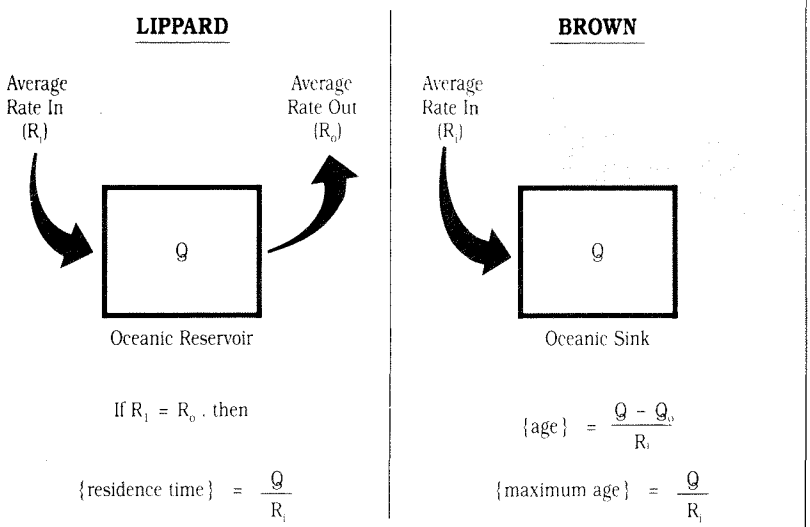

FIGURE 1.

Comparison of Two Models Concerning the Concentration of Certain Metals in the Oceans

Note: Considerable error could result if today's input rate is used to approximate the average input rate (R_i).

ference. Which model is correct?

I maintain that the oceans are a sink for most metals ($R_o = 0$); that is, no significant quantities of those metals are leaving the oceans by absorption, precipitation, or organic removal. Most metals and their least soluble compounds are undersaturated in the oceans (Riley and Skirrow, 1975). This both prevents their precipitation and enhances their tendency for re-solution. Saturation levels should have been easily reached over "geologic time scales."

Lippard cites the wide divergence in "residence times" for various metals. His very low values are obviously explained by the high influx of industrial metals (aluminum, titanium, and iron) into the oceans since the beginning of the Industrial Revolution. I contend that Lippard's very high values for sodium, potassium, and so forth, are the result of their high initial concentration ($Q_o \gg 0$). Why these initial concentrations were high is explained in the hydroplate theory (Brown, 1989c, pp. 58–83).

Lippard says that these metals are leaving the ocean at about the same rate they enter ($R_o = R_i$). Who has the burden of proof? Obviously Lippard, because I claim there is nothing to measure. But Lippard can't point to any large measure-

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ments of R_o, even on the ocean floor. His excuse is that these elements "are subducted into the mantle of the earth at various locations" and therefore cannot be measured. How convenient. Is subduction truly occurring?

The earth's crust is composed of a dozen or so plates, each about thirty miles (fifty kilometers) thick. The plate tectonic theory, which Lippard and most evolutionists accept, claims that some of these plates dive, or *subduct*, under the edge of an adjacent plate along oceanic trenches. The physics of subduction is never clearly explained. In fact, I claim that forces do not exist to cause subduction. If Lippard (or anyone else) disagrees, I would like to see a free-body diagram of this process showing all relevant forces, including friction. Lippard should also explain the initiating mechanism and describe quantitatively the energy that drives the movement. The hydroplate theory explains how trenches formed, why so many earthquakes occur beneath them, and other aspects of trenches and earthquakes that contradict the plate tectonics theory.

Lippard: "Furthermore, Brown's statement, 'There is no known means by which large amounts of these elements can precipitate out of the oceans,' is simply false. Nickel, for example, may be taken up by ion exchange into clay minerals, substitutes for magnesium in crystal lattices, and forms insoluble hydroxides in alkaline solutions. . . ."

I spoke of no significant precipitation; Lippard countered with examples of minor absorption. He is correct in saying that a few metals, such as nickel, can come out of solution—but only in small amounts which probably are balanced by resolution. In any event, the ocean floor does not hold the quantity of all of these metals that Lippard requires.

Shrinking sun.

Lippard: "In category ninety-two, Brown claims that the sun is shrinking at a constant rate. . . ."

No, I didn't. There are certain slight oscillations in the sun's diameter. The key question is whether or not those oscillations are superimposed on a diameter whose long-term trend is one of shrinkage. Eddy and Boornazian (1979) reported that, over at least a ninety-year period, the sun shrank. The mere fact that so many studies have since addressed this matter should make it clear that this is a legitimate, current, and critical question. If there is a shrinkage trend, it is presumably driven by gravitational forces. Had the sun existed millions of years ago, it would probably have been so large that it would have heated the earth to such an extent that life could not have survived. I have read at least twenty-one original studies that bear on this question, including the select few that Lippard mentions. To get into

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the methodology, accuracy, and relevance of even a few would be tedious and inconclusive for most readers, although I feel that the best data supports a slight but significant shrinkage trend.

There is one compelling line of evidence for a shrinking sun. Nuclear fusion in the sun produces neutrinos. Many very elaborate and expensive experiments have found that the neutrino flux is at best only one-third of what it should be if all the sun's heat is caused by fusion. These experiments have been repeated and the computations checked by hundreds of scientists. This is considered one of the biggest surprises in science during the past forty years. If only one-third of the sun's heat is produced by nuclear fusion (as the data implies), then where does the rest of the heat come from?

Prior to the discovery of nuclear fusion, Helmholtz in 1854 and Kelvin in 1861 proposed that the sun's energy comes from gravitational contraction. As the sun compresses, it heats up and radiates its diminishing potential energy as sunlight. The maximum potential energy of the sun establishes an upper limit on the age of the sun (and the solar system) of several million years. Later, when nuclear fusion was discovered, it was seized upon as a source of all the sun's energy, effectively removing the sun's age limit and giving vastly more time for evolution to happen. If we must now replace two-thirds of that upper limit, then the sun cannot be billions of years old, and there would not have been enough time for evolution to have occurred.

Incidentally, Jupiter and Saturn each radiate more than twice the energy they receive from the sun. Where is this excess heat coming from? Not from nuclear "fires," because those planets are not massive enough to initiate nuclear fusion, and because their densities are too low for there to be much material that could cause nuclear fission. Heat should not be released by liquifying the hydrogen and helium gases that make up Jupiter and Saturn, since the temperatures of those planets are above the critical temperatures for hydrogen and helium. (The *critical temperature* of a gas is that temperature above which no amount of pressure can squeeze it into a liquid.) I know of only two other possibilities: either gravitational contraction is occurring or those planets have not lost their initial heat. Certainly, if they have been sitting in cold, dark space for billions of years, they would have lost their initial heat. Either way, the fact that those planets are relatively hot suggests that they are young.

Other clocks. Again, Lippard alludes to refuting evidence but describes and explains nothing.

Two- to Twenty-Celled Life Forms

I understand what Lippard is saying, but he missed the conundrum I raised. How did the first multicellular life—animals, plants, or anything else—evolve? An evolutionist would obviously say, "From unicellular life." Fine. But what followed

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single-celled life? Certainly not Mesozoa, because they are parasites and must have a complex multicellular host to provide digestion and respiration. Certainly not colonial forms, for reasons I gave in my first article. The simplest multicellular form of life, excluding parasites and colonial forms, probably has over ten thousand cells. What transitional forms evolved to fill the huge gap between one and ten thousand cells? Evolutionists don't appear to have an answer.

Archaeopteryx

Hoyle and Wickramasinghe make a good case that the two fossils of *Archaeopteryx* that have clearly visible feathers are forgeries (1986). These two fossils (the London specimen of 1861 and the Berlin specimen of 1871) were "found" and sold at very lucrative prices by a father and his son. Hoyle alleges that thin layers of cement were spread on two fossils of the small dinosaur, *Compsognathus*. Bird feathers were then imprinted onto the wet cement.

Hoyle's and Wickramasinghe's book contains clear color photographs showing several examples of where the coloration, shape, and texture on the main slab and counterslab do not match; feathers that are too flat and lie too precisely at the slab-counterslab interface; and an upside-down furcula. A furcula is diagnostic of a bird. Since it is upside down and is visible on only the London specimen, it may have been carved to create the false impression that *Archaeopteryx* was a bird.

The "anti-hoax" articles Lippard cites either don't address most of these evidences or else contain statements which are shown to be false by Hoyle's photographs. Lippard cites no contrary evidence but resorts to derogatory comments: "falsehoods," "absurdity," "ignorance," "ludicrous," "contempt," and so forth.

Lippard: "But Brown's comment implies that he now no longer thinks the hoax hypothesis is a good one."

This is a misrepresentation. What I have said, since 1986, is:

> Even if *Archaeopteryx* is not a forgery, it cannot be ancestral to modern birds since the fossils of two modern birds have been found in rock strata dated by evolutionists as much older than *Archaeopteryx*. For details see: Tim Beardsley, "Fossil Bird Shakes Evolutionary Hypotheses," *Nature*, Vol. 322, 21 August 1986, p. 677.

By the way, would any paleontologist who thinks that *Archaeopteryx* is a bird tell me how it was buried in the ultra-fine-grained, 80 percent pure, Solnhofen limestone? Why is there so much limestone on the earth, and what is the source of all the required calcium and carbon? In just the Bahamas, the limestone is three

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miles thick! "The Fountains of the Great Deep" contains my explanation for the deposition of most of the earth's limestone and how organisms in general were fossilized in it (1989c, pp. 59–83).

Lippard: ". . . Brown knows there is now another specimen with clear feather impressions."

Lippard is again vague. He is apparently referring to the Solnhofen specimen. No one knows who found it or when or where it was excavated. Someone has tampered with it, at least to the extent of carving out a longer tail (Wellnhofer, 1988). From its photograph, I cannot see any feathers or feather shafts. Wellnhofer says they are there, although he must resort to a sketch to show readers where he thinks they are. Since the significance of all the other *Archaeopteryx* fossils may rest upon whether or not the Solnhofen specimen has feathers, why didn't Wellnhofer's paper have a high-resolution photograph showing details of a feather, such as a barbule?

Lippard: "Regarding Brown's other argument (that Protoavis *destroys the status of* Archaeopteryx *as a transitional fossil): while* Protoavis, *if a bird, would probably eliminate* Archaeopteryx *as an ancestor to modern birds, the identification of* Protoavis *as a primitive bird is not yet generally accepted. . . ."*

Of course, *Protoavis* has not yet been accepted as a bird by some evolutionists. To do so would eliminate their best example of an embarrassingly few possible transitional forms. Lippard gives no reasons for rejecting *Protoavis*. *Protoavis* has a distinct furcula, a small keel, a platelike sternum, and other characteristics that are diagnostic of birds. It is seventy-five million years older (by evolutionists' reckonings) than *Archaeopteryx*. Therefore, *Archaeopteryx* cannot be the first bird.

Lippard's reference to Cuffy is one more example of his claiming that someone has some evidence which Lippard cannot discuss.

Human Evolution

Lippard: "Regarding Charles Oxnard's studies of australopithecines, Brown's response ignores a significant portion of my comment—that these studies did not take into account Donald Johanson's 'Lucy' find or any examples of Australopithecus afarensis. *To Brown's credit, he has changed the latest edition of his book to make note of this fact in a single sentence of a footnote. . . ."*

Wrong again. That statement was in an earlier edition of *In the Beginning* (Brown, 1986, p. 25) as well as the reproduced section from that book that I gave to Lippard on January 22, 1987. I cannot include in a short article everything that is in my book.

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Lippard: "Citing Oxnard in support of the claim that no australopithecines are in the human lineage is extremely misleading, because Oxnard himself states that Australopithecus afarensis *is probably in the human lineage. He states that, if scientists will use the term* australopithecine *(which has indeed become the case) for finds at East Turkana, Laetoli, and the Afar Valley (for example,* Australopithecus afarensis *in the latter two locations), 'the new usage of the term will be for these other fossils labeled "Homo?" in figure 18' (Oxnard, 1979, p. 274)."*

While Mary Leakey's Laetoli footprints are usually attributed to *Australopithecus afarensis*, their details are indistinguishable from those of humans. Nothing else at Laetoli was close to being human. Why not conclude that humans made those footprints? Evolutionists don't reach that conclusion because humans would appear "too early" for their evolutionist timetable.

Oxnard never even used the word *afarensis* in the paper Lippard referenced. Oxnard has said in dozens of papers, including the one Lippard referenced, that the australopithecines are so distinct that they do not appear to be in the human lineage, that they appear to be a parallel development having nothing to do with humans, and that if old *Homo* fossils are found their discoverers might mistakenly call them australopithecines. Oxnard warns people not to accept the widely published stories that the australopithecines are in the human line, since those conclusions are based only upon visual techniques and human biases. Oxnard had not then analyzed the *afarensis* bones with his powerful multivariate techniques. He guessed that recent discoveries might turn out to be *Homo* instead of *Australopithecus*, but he tempered his statements with many question marks. Here is just a small sampling of what Oxnard has really said:

> The genus *Homo* may, in fact, be so ancient as to parallel entirely the genus *Australopithecus* thus denying the latter a direct place in the human lineage.
> [1975, p. 389]

> . . . [while] the conventional wisdom is that the australopithecine fragments are generally rather similar to humans and when different deviate somewhat towards the condition in the African apes, the new studies point to different conclusions. The new investigations suggest that the fossil fragments are usually uniquely different from any living form; when they do have similarities with living species, they are as often as not reminiscent of the orangutan.
> [1979, p. 273]

In Oxnard's latest book, he suggests that no australopithecine, not even *Australopithecus afarensis*, is ancestral to humans. He even cites a paper by Bernard Wood which states that "no australopithecine currently known could have been a human ancestor!" Oxnard goes on to say, "The last decade has now, however, seen this

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notion become generally accepted. The various australopithecines [and that would include *afarensis*] are, indeed, more different from both African apes and humans in most features than these latter are from each other" (1987, pp. xi, 227).

Lippard: ". . . Oxnard says that Ramapithecus *may well be a human ancestor . . . (Wu and Oxnard, 1983)."*

No, he didn't. That paper was a statistical study of some dimensions of nine hundred teeth of *Ramapithecus* and *Sivapithecus* found in China. Oxnard and Wu were trying to identify any sexual dimorphism and just how many species were present. At several points, the authors only said that some of those statistics were similar to humans. Oxnard's most recent conclusion on the matter is that "all non-human Asian fossils (ramapithecines, *Gigantopithecus*) are on or close to ape lineages" (Oxnard, 1987, p. 219).

Lippard also implied that Oxnard used his multivariate techniques in reaching his conclusions about *Ramapithecus*. He didn't.

Lucy and Bonzo

Lippard: "Brown should read Johanson's writings before making sweeping claims about what they do or don't say."

Johanson has not addressed the implications of the vertical scattering of his 240 to 350 *Australopithecus afarensis* bone fragments. These bones consist primarily of a knee joint, the "First Family," and Lucy (a sixty-pound, tiny-brained, long-armed, 3.5-foot tall, adult primate with nearly 40 percent of her fractured bones remaining). One hundred meters separated Lucy and a knee joint, which, let's say, belonged to Bonzo, a member of the same species. (The name "Lucy" sounds human, unjustifiably; so I will balance that with the name "Bonzo.") To my knowledge, Johanson has never presented a reasonable scenario as to why a piece of Bonzo, bone fragments of many cousins, and parts of Lucy span one hundred meters of very interesting strata. Had Johanson squarely addressed this issue, enough controversy and doubt would have been raised that I would have heard about it. Let me explain.

According to Johanson, those one hundred meters represent about eight hundred thousand years of time. Is it reasonable that these *afarensis* primates lived in the Afar Triangle at various times over an eight hundred thousand year period? During that time, the sea must have transgressed and egressed that region several times. Carbonate and gastropod-bearing horizons were deposited along with an ostracod-rich horizon. Major faulting and other catastrophic events also occurred during that deposition process (Johanson et al., 1982). Isn't it strange that Lucy

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came back to the old homestead where Bonzo and her cousins had lived many hundreds of thousands of years earlier? Why haven't more bones of *Australopithecus afarensis* been found elsewhere if several hundred thousand generations of these *afarensis* primates had so much time to migrate? And why are the primate bones at Hadar so fractured and scattered?

Let me propose another hypothesis. A colony of apelike animals, which included Lucy and Bonzo, was overcome by a relatively recent, catastrophic flood that was accompanied by major tectonic and volcanic activity. Their dismembered bodies, and some gastropods and ostracods, were sorted out in those carbonate-rich, muddy waters in such a way that pieces of their skeletons now span one hundred meters of strata. Their broken and scattered bones certainly imply some violent event that engulfed many, and their well-preserved bones certainly belie a three-million-year age. (For a more complete description of the setting and reasons for this hypothesis, *see* Brown, 1989c, pp. 58–83).

How can we test these two hypotheses? Let's encourage the Ethiopian officials to let us radiocarbon date just one of the 240 to 350 *Australopithecus afarensis* bones. By using an atomic mass spectrometer (AMS), only a tiny fraction of the bone would be destroyed. If Johanson is correct and *Australopithecus afarensis* is over three million years old, no radiocarbon will be found. If my hypothesis is correct, radiocarbon will be found.

Lippard falsely stated that Carl Baugh "has consistently refused" to have the handle of his apparently ancient hammer radiocarbon dated (Lippard, 1989, p. 31). Plans for such a test are now proceeding, thanks to R. E. Taylor, the director of the Radiocarbon Laboratory at the University of California at Riverside. I have asked that this be a blind test. In other words, several other miscellaneous specimens and the wood from Baugh's hammer will each be reduced to "a pinch" of carbon powder. Then an independent third party will secretly relabel the specimens and only reveal the true labels after the radiocarbon dates have been established. Let's add an *afarensis* (or some other australopithecine) specimen to that blind test. If Baugh and the owners of the Shroud of Turin are willing to subject their specimens to a blind, radiocarbon AMS test, shouldn't those involved with "ancient" hominids do likewise? Most scientists consider blind tests a standard procedure for overcoming experimenter bias. Why don't evolutionists—especially when using radiometric dating?

Lippard: "Brown tries to use the controversy over the nature of Lucy's bipedality to show that Lucy was not bipedal at all. . . ."

My position has consistently been that Lucy probably did not walk upright in a human manner. Her bodily proportions and bone structure suggest that her gait was similar to that of a pygmy chimpanzee. Furthermore, her long arms and curved finger and toe bones indicate that she swung from the trees.

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Lippard: "Brown does not specify what evidence he thinks was withheld [that showed that Java 'man' was a large gibbon]" (1989, p. 27). "Regarding my criticism of his descriptions of Java Man, Brown has no response" (1990, p. 28).

I didn't discuss that in my 1989 article, but it was in my book that Lippard had before the article was published. I will repeat it for his benefit:

> Eugene Dubois acknowledged forty years after he discovered Java "man" that it was probably just a large gibbon. Dubois also admitted that he had withheld parts of four other thigh bones of apes, found in the same area, that supported that conclusion. [Brown, 1989c, p. 5]

The text gives six supporting references and an admission by Dubois.

My previous comments concerning Castenedolo, Calaveras, Neandertal, and Noah's ark stand (1989b).

Conclusion

Lippard: "As I noted in my original article, the points I addressed therein were simply a selection intended to give the flavor of Brown's work. There was no attempt to find the 'weakest' of his points."

Do you, the reader, really believe that?

You have seen Lippard make other inaccurate statements, twist my words, display his bias in many ways, repeat (without any fundamental understanding) erroneous arguments that he drew from various sources, and shallowly deal with scientific matters that he himself raised. One leading anticreationist told me that he felt that Lippard was unqualified to participate in this written exchange. You may wonder if someone other than a graduate student in philosophy could have done better. Certainly. Here are fifty: Isaac Asimov, Frank Awbrey, William Bennetta, C. Loring Brace, Stephen Brush, Catherine Callaghan, Preston Cloud, John Cole, Brent Dalrymple, Bob Demar, Bob Dietz, Russell Doolittle, Vincent D'Orazio, James Ebert, Niles Eldridge, Douglas Futuyma, Owen Gingerich, Laurie Godfrey, Stephen Jay Gould, L. Beverly Halstead, Phillip Hewitt, Jim Hopson, Donald Hornig, Philip Kitchner, John Lynch, Lew Mammel, William Mayer, Ian MacGregor, Chris McGowan, Kenneth Miller, David Milne, Ashley Montagu, Dorothy Nelkin, Craig Nelson, Fred Parish, John Patterson, Ronald Pine, David Raup, Bob Richards, Joseph Roll, Michael Ruse, Carl Sagan, Vince Sarich, Steven Shore, Frank Sonleitner, Arthur Strahler, William Thwaites, Howard Van Till, Stanley Weinberg, and Michael Zimmerman.

If just a few of these people were willing to participate in a book-length scien-

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tific debate, then much of this origins controversy could be clarified. Tens of thousands of teachers, and many others, would like to read what both sides have to say about radiocarbon dating, interesting fossils, the likelihood that mutations could produce increasing complexity, many dating techniques, dinosaurs, many geologic matters, and a hundred other topics. Lippard said that "holders of doctorates . . . are less likely to have the time or desire to debate Brown." That won't wash. Each person above has devoted much time trying to attack the creationist case. This list could be multiplied many times.

I'll lay it on the line. Leading anticreationists have everything to lose and nothing to gain by engaging in an extensive written debate. It's hard to retract things that are in print. Their careers and reputations would be jeopardized, and many of their evolutionist colleagues would be angry with them for opening up evolution for criticism. Furthermore, qualified anticreationists have everything to gain and nothing to lose by having someone like Lippard "enter the ring." In the meantime, you (the reader) are not able to see if the evolution case can really stand head-to-head with the creation case. These individuals ought to be ashamed.

Most readers of *Creation/Evolution* are evolutionists. I was too until about twenty years ago when I began studying the evidences dealing with origins. After learning of just a few of the 127 categories of evidence, I was curious enough to study in various fields that I felt held the evidence for evolution. I found very little. Instead, there was more and more evidence for creation and a recent, global flood. Since then, I have been appalled at what is and has been taught concerning origins.

What are most scientific creationists advocating? As I see it, three things: (1) that no religious doctrine or writing be taught or ridiculed in the public schools; (2) that all the major scientific evidence dealing with origins be brought out at the appropriate grade levels; and (3) that when a theory of origins is presented, any opposing evidence must also be presented. Knowledgeable and honest evolutionists must admit that there are many strange observations that do not fit evolutionism. If they don't know of any, they are not on the "cutting edge" or they haven't looked in the right places. The history of science shows repeatedly that scientific advancement usually results from a surprising observation that challenges current thinking. The new explanation is usually resisted by those who have a stake in the current thinking—those who teach it and those who derive income, power, and prestige from it. Students and laypeople who have no desire or ability to examine the new evidence naturally tend to accept the prevailing view. The desire to conform and be accepted is powerful, especially in academia. Those who want to appear knowledgeable will spout the prevailing "wisdom." The more people ally themselves with the old thinking, the greater the contest becomes, as long as the new explanation uniquely explains the scientific anomaly. If the change in thinking has philosophical, social, or religious implications, the issue will become very emotional. Courts, legislatures, and committees may be drawn into the battle by the non-

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scientific elements of each side. Neither they nor money and verbiage will settle the issue one way or the other, although they may delay its resolution, raise the stakes, attract attention, and ignite tempers. Only by dealing with the scientific anomaly and all possible explanations can the matter be resolved. In other words, the challenge to the old thinking will ultimately rise or fall on the scientific evidence. Ignoring the evidence is bad enough; misrepresenting, dismissing out of hand, and censoring one's opposition is even more serious; but failing to tell students about it betrays a trust.

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Letters to the Editor

In "Brown Responds to Lippard" (*Creation/Evolution* XXV), Walter Brown makes a number of claims about secular changes in the values of fundamental constants of physics. Since much of what he says is misleading and some of it is wrong, a response is in order.

Brown quotes Troitskii as supporting a decay in the speed of light *c*. However, Troitskii is talking about decay rates that are a million times smaller than young-earth creationists require, so this does not help their case. A careful reading of Troitskii's paper shows that any decay in *c* must be accompanied by comparable changes in other fundamental constants, such as the fine structure constant α. Troitskii points out that such changes would be detectable after only ten years (he assumes that the universe is at least ten billion years old). But if the universe is only ten thousand years old, as young-earth creationists say, the corresponding rates of change in these other fundamental constants would have to be a million times larger than Troitskii assumes. Such large changes would already have been observed, for they are many orders of magnitude larger than the detectability limit. Since they have not been observed, on these grounds alone we can say that Troitskii's paper in no way supports creationist theory and, in fact, contradicts it.

Brown discusses Van Flandern's work on variations in the gravitational constant *G*. Here there are two points to make. First, dynamical astronomers universally recognize that these observations are very difficult to make and interpret (T. Damour, G. W. Gibbons and J. H. Taylor, "Limits on the Variability of *G* Using Binary-Pulsar Data," *Physical Review Letters*, 1988: 61:1151). Although Van Flandern believed that he had detected a very small secular change in *G*, the consensus of nearly all workers in this field is that Van Flandern's data do not support this conclusion. At best, he has set an upper limit, which means that, to within the measurement accuracy, *G* does not change (R. D. Reasenberg, "The Constancy of *G* and Other Gravitational Experiments," *Philosophical Transactions of the Royal Society of London*, 1983:A310:227–238). The second point is similar to the one I made above in connection with Troitskii's work. Even if we were to accept Van Flandern's value at face value, the *rate* at which *G* changed would be extremely small and would only become significant over the ten-billion-year lifespan he presumed for the universe. In order to obtain the change creationists require in only ten thousand years, the rate of change of *G* would have to be a million times larger than Van

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Flandern's value. Such a large change is clearly ruled out by observation.

Like many others, Brown speculates on changes in radioactive decay rates. Here again, creationist speculation must contend with some hard facts. Assuming constant decay rates, the radiochronometric data on terrestrial and lunar rocks show convincingly that the solar system is between four and five billion years old. In order for these data to be consistent with young-earth creationism, over the past ten thousand years the decay rates must have *averaged* five hundred thousand times larger than they are now and the *maximum* decay rates must have been many orders of magnitude larger than that. Such large decay rates would have been accompanied by a correspondingly large release of energy. If this had happened, the earth would still be molten, and the enormous background radiation would have destroyed most if not all life. Furthermore, as Brown states, a decay in c means that atomic properties would change. What he does not seem to realize is that, for c to have been large enough in the past to support his scenario, atomic properties would have had to have changed very radically indeed. We know that the fundamental constants of physics are so delicately balanced that even very small changes in them would make life impossible (W. H. Press and A. P. Lightman, "Dependence of Macrophysical Constants on the Values of the Fundamental Constants," *Philosophical Transactions of the Royal Society of London*, 1983:A310: 323–336). Since the creationist c-decay theory requires changes in fundamental constants that are many, many orders of magnitude larger than what is permitted, it must be rejected.

Brown discusses the fact that there are more superluminal jets in quasars than naive theory would predict. He says that as a result "the relativistic explanation is generally rejected." This is absolutely wrong. The apparent excess of superluminal jets is easily explained if quasars are systematically brighter, and hence more readily detected, when their jets are beamed toward us, just as a flashlight is much brighter when pointed directly toward an observer (D. Wills, "Rapid Radio Emissions," *Science*, 1987:238: 1740). There is convincing evidence that this is the case and that it does indeed explain the observed excess. The generally accepted explanation of superluminal jets is still that it is a relativistic effect (T. J. Pearson and J. A. Zensus in *Superluminal Radio Sources*, Cambridge University Press, 1987:6; M. H. Cohen, "Radio Sources: Small Scale Structure," in *Active Galactic Nuclei*, Berlin: Springer-Verlag, 1988:296–300.

Finally, Brown discusses the work of Pappas and Oblensky. Since their work is highly controversial and has not yet been replicated, any conclusions based upon it are purely speculative. At this time, it certainly does not constitute credible evidence for a decay in c.

I thank G. Shields, E. Vishniac, B. Wills, and D. Wills for useful discussions.

—William H. Jefferys

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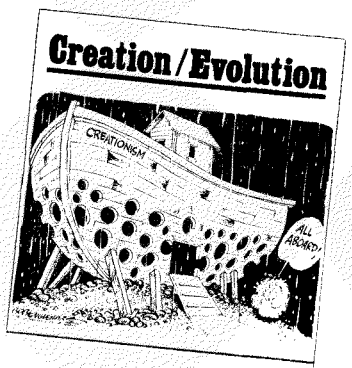

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